THE

ANNALS

OF

PHILOSOPHY.

NEW SERIES.

JULY TO DECEMBER, 1823.

VOL. VI.

AND TWENTY-SECOND FROM THE COMMENCEMENT.

London:

Printed by C. Baldwin, New Bridge-street;

FOR BALDWIN, CRADOCK, AND JOY,

PATERNOSTER-ROW.

1823.

TABLE OF CONTENTS.

NUMBER I.—JULY.

NUMBER II.—AUGUST.

NUMBER III.—SEPTEMBER.

NUMBER IV.—OCTOBER.

NUMBER V.—NOVEMBER.

NUMBER VI.—DECEMBER.

PLATES IN VOL. VI. (*New Series.*)

ERRATA.

Page 82,	line 22,	*for* vibrate, *read* oscillate.
84,	4,	*for* parallelopedal, *read* parallopiped.
85,	29,	*for* quantity, *read* quality.
88,	5,	*for* parallelium, *read* parallelism.
—	27,	*for* harp-unisons, *read* harp, unisons.
89,	7,	*for* repulsing, *read* repelling.
90,	24,	*for* place, *read* plane.
95,	6,	*for* Baden Bowell, MA. *read* Baden Powell, MA.
215,	14,	from bottom, *for* 100 feet, *read* 1000 feet.
218,	14,	*for* Dex, *read* Dax.
232,	15,	*for* New England, *read* Connecticut.
299,	10,	*for* alternatis, *read* alternis.
371,	6,	*for* mean of result, *read* mean of his result.
—	24,	*for* potash, *read* alkali.

The notice respecting the Correctness of the Greenwich Observations inserted in p. 397, and signed X. should have had the signature of "James South, Blackman-street."

ANNALS

OF

PHILOSOPHY.

JULY, 1823.

Article I.

Observations on Sir W. Congreve's Report on Gas Light Establishments. By M. Ricardo, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Brighton, May* 18, 1823.

I HAVE been favoured with a copy of the Report of the Royal Society, and two additional Reports of Sir W. Congreve to the Secretary of State of the Home Department, which were laid before the House of Commons, and ordered to be printed, and on which I beg to offer some few observations through the medium of your journal. The first Report, which was signed by some of the leading members of the Society, was made nine years ago (1814), in consequence of an inquiry being instituted to ascertain the probable danger of gas light establishments; the second and third Reports by Sir W. Congreve were delivered in Jan. 1822 and 1823. This gentleman has not confined himself to noticing the dangers which are likely to arise from the diffusion of this mode of lighting, and pointing out what he thinks are the best methods of avoiding them; but he has also entered somewhat fully into the nature and management of the various companies, and has thrown out some hints for legislative enactments to regulate their future conduct, of which I shall take some notice hereafter.

Sir W. Congreve has enriched the Report with some tables of the proceedings of the three principal Companies:—the three stations of the Chartered Company, the City of London Com-

pany, and the South London Company. When I first saw these, I expected to derive some valuable information from them, which would enable me to come to more correct conclusions in my inquiries relative to the comparison between oil and coal gas, but I have been sadly disappointed. A slight examination soon proved to me that the statements which they contain could not be at all depended on, and I was, therefore, led to enter into a more minute analysis of them. I endeavoured, if possible, to account for the very different results which appeared in the different Companies, but with very little satisfaction, as I cannot come to any conclusions that can be relied on. Although this is a subject which I fear will not afford much entertainment to your readers, nor do I think that the private transactions of Companies are fit subjects for public investigation, yet I am induced to send you the result of my inquiries in as concise a form as possible, and chiefly so, as Sir W. Congreve has founded his Reports on these statements, and no doubt relying on their correctness, has thought it necessary to recommend legislative enactments upon them.

In imitation of Sir W. Congreve's plan, I have also annexed a table of the proceedings of the different Companies, part of which is borrowed from his, and the remainder, the results of my own calculations. In the progress of my examination, the observations so accumulated upon me, that I was anxious to devise some mode for putting them in a concise but yet intelligible form, and I saw no better method by which it could be effected than the one I have adopted. Here the whole management of the different works, together with the very different results, may be seen at one view, and any of your readers who, like myself, may be interested in the subject, will be able to form their own judgment as to the probable correctness of the statements.

The information which I was chiefly desirous of obtaining, was the quantity of gas that was consumed by a given number of lights, the quantity that was wasted or lost, the capital that is employed, the cost in labour, wear and tear, and management, the profit, &c. but these will be found to vary so much that it will be impossible to come to any correct conclusion.

With regard to the quantity of gas consumed by the different Companies, the mode by which that is estimated in the tables in the Report is so obviously incorrect, that I have adopted another method in order to ascertain it, which, though liable to error, is certainly a nearer approximation than the other. The lights in the Report are divided into two sorts, private lights and public lights; the private lights are stated to burn upon an average throughout the year; that is, for 313 days, excluding Sundays, one with another, four hours per night, consuming, by one Company, $4\frac{1}{2}$ cubic feet; another $6\frac{1}{2}$, and another 6 feet per hour: the public lights are stated to burn nine hours per night for 365 nights, consuming the same quantity of

gas per hour: now among the public lights are estimated what I have termed occasional lights, such as are used at the theatres, public bodies, churches, meeting houses, &c. which, upon an average, consume a much smaller quantity of gas than the private lights, instead of equalling the public. In the Westminster station, the number of these occasional lights is stated, the private lights being 10,660, the public or street ditto 2,248, and the occasional 3,894. In the other stations, the number of public bodies is given without stating the number of lights. I have, therefore, assumed they are only one half; the whole number, therefore, in the Chartered Company is 21,886 private, 3,452 public, and 5,097 occasional lights, for which a rental is paid of 125,977*l.* According to the rate of charges, a gas light burning from sunset till nine o'clock, pays 4*l.* per annum. I have estimated in a former paper that this upon an average burns for 20 hours per week, the estimate in the report is four hours per night, or 24 hours per week. This extra allowance will account for those lights which extend beyond nine o'clock, and for which an extra charge is made. The nearest approximation then to an average charge would be for each private light, 4*l.* 4*s.* The average consumption of each burner where experiments have been tried has always been stated to be 5 feet per hour, and it is upon this quantity and price I have founded my calculation. A private light burning four hours, 5 feet per hour, consumes 20 feet per night, which, multiplied by 313, the number of days, amount to 6,260, which again multiplied by 21,886, the number of lights, will give the whole quantity consumed by the private at 137,006,360 feet, and at 4*l.* 4*s.* per light, the rental of these will amount to 91,921*l.* or about 13*s.* 6*d.* per 1000 feet.

The public or street lights are I understand usually charged at 5*l.* 5*s.* each; they are estimated to burn the same quantity as the private lights, and the average time of burning per night throughout the year is nearer ten than nine hours, the nightly consumption of each light then will average 50 feet for 365 nights, and the annual 18,250, which, multiplied by 3,552, the number of street lights, give 64,824,000. At 5*l.* 5*s.* per light, the rental of the public lights will amount to 17,981, or about 5*s.* 9*d.* per 1000 feet.

The rental then for the private and public lights will amount to 109,902*l.* which, deducted from the whole rental 125,977*l.* leaves for occasional lights 16,075*l.* We may consider that the charge for these lights will be at the same rate as the private lights, or 13*s.* 6*d.* per 1000 feet, which would give a consumption for the above-named sum, of 23,814,000 feet. By this mode of calculating, the whole consumption of gas will amount to 225,644,360, leaving a deficiency for waste of 22,499,640, or nearly 10 per cent. I have by the same method estimated the consumption of the City of London Company, and the South London Company.

South London Company.	City of London Company.	Chartered Company. The whole.	Chartered Company. Curtain Road Station.	Chartered Company. Brick Lane Station.	Chartered Company. Westminster Station.	
3640	8840	20,678	3336	8060	9282	*Number of chaldrons of coals used.
60	170	341	55	133	153	*Average number of retorts in use.
88	210	518	80	217	221	*Greatest number in use at one time.
115,675	181,282	627,638	107,122	221,131	309,385	*Capacity of all gasometers in feet.
176,322	360,720	1,038,072	160,000	437,000	442,143	*Greatest quantity of gas produced in one day.
43,680,000	106,080,000	248,144,000	40,040,000	96,720,000	111,384,000	*Quantity of gas made in the year.
3538	5423	21,886	3860	7366	10,660	*Number of private lights.
500	1412	3552	315	989	2248	*Number of public lights.
500	1412	5097	315	989	3894	*Number of occasional lights.
35	50	125	25	40	60	*Number of miles of main.
86	108	172	154	184	178	Number of private lights per mile.
14	24	27½	12½	25	37½	Number of public lights per mile.
14	24	40	12½	25	64¼	Number of occasional lights per mile.
19,017,880	33,966,760	137,006,360	24,163,600	46,111,160	66,731,600	Quantity of gas consumed for private lights.
9,225,000	22,100,750	64,824,000	5,748,750	18,049,250	41,026,000	Quantity of gas consumed for public lights.
None	2,183,000	23,814,000	2,988,550	6,635,500	9,539,100	Quantity of gas for occasional lights.
15,447,920	47,829,490	22,499,640	7,339,100	25,924,090	—	*Waste of gas.
—	—	—	—	—	5,612,900	Minus quantity of gas.
£96,000	£131,250	£580,000	—	—	—	*Capital expended.
7½ per cent	7 per cent.	8 per cent.	—	—	—	*Dividend on capital.
£26 7*s.*	£14 16*s.*	£28	—	—	—	Capital on each chaldron of coals.
£14,963	£30,839	£125,977	—	—	—	*Rental.
£5,460	£13,260	£31,017	—	—	—	Cost of coals, deducting sale of coke.
£2,302	£7,839	£49,060	—	—	—	Labour, wear and tear, management, &c.
£7,200	£10,240	£46,400	—	—	—	Profit.
£4 2*s.* 3*d.*	£3 10*s.* 0*d.*	£6 1*s.* 10*d.*	—	—	—	Rental on each chaldron.
£1 10*s.* 0*d.*	£1 10*s.* 0*d.*	£1 10*s.* 0*d.*	—	—	—	Cost of a chaldron of coals, deducting sale of coke.
£0 12*s.* 9*d.*	£0 17*s.* 9*d.*	£2 7*s.* 11*d.*	—	—	—	Labour, management, wear and tear, &c.
£1 19*s.* 6*d.*	£1 2*s.* 3*d.*	£2 3*s.* 11*d.*	—	—	—	Profit on each chaldron.
38 per cent.	36 per cent.	38 per cent.	—	—	—	*Premium on shares.

Those columns marked thus (*), are copied from the tables in the Report; the others are from my own calculations.

My first observations will be directed to the quantity of gas produced and consumed, and here we not only observe a very considerable variation in each Company, but also a very marked difference in the different stations of the same Company. At the Westminster station, it will be found the whole consumption of gas for all lights by my mode of calculating leaves a minus quantity of 5,612,900, which would be considerably more if estimated, as they have done all, as public lights. At the Brick Lane station, the overplus or waste is above 21 per cent.; and in the Curtain Road, nearly 20 per cent. At the City of London Road Works, the waste will be found to be nearly 46 per cent. and at the South London 38 per cent. It will be impossible to account for the extraordinary difference which exists in these statements, but by supposing there must be some error: but the most surprising discrepancy is in the Westminster. The proportion is so different from either of the others, I should be very strongly inclined to think that the quantity of gas produced from a given quantity of coal varied very materially, although it is stated that at each station one chaldron of coals produces 12,000 feet of gas. Unless they have an accurate gas-meter through which all their gas enters as it is made, previous to its passing into the gasometers, I know not by what means they can possibly ascertain what quantity of gas is made, as at times, particularly in the long nights, they must be producing and delivering at the same time; to assume that, because a chaldron of coals has upon one or two trials produced 12,000 feet of gas, it must always produce the same quantity, is certainly a very imperfect datum to calculate upon. At the City of London Works, where it is stated the greatest waste takes place, there are strong grounds for presuming that they over-calculate the quantity of gas produced. By the tables in the Report, a chaldron of coals is stated in all the Companies to yield the same quantity of gas and the same quantity of coke. The Chartered and South London give in addition 10 gallons of tar and 11 gallons of ammoniacal liquor, as it is there termed, while the City of London Works produce 16 gallons of the former, and 18 gallons of the latter. Now it is not very probable that a chaldron of coals in their hands should obtain an excess of 13 gallons of two products without any diminution of the others. The more likely supposition is, that if there be this excess in these, there must be a corresponding deficiency in the other; it is on this account that I have made my calculations upon the quantity of coals used, and not upon the quantity of gas produced. In the one case it is most probable they are correct; while, in the other, their accuracy is more than doubtful. In examining the tables, we are struck with the very great advantages which the Chartered Company possesses over the other two. As we are oftentimes puzzled by the exhibition of a large number of figures, and do not readily see the exact proportions, I have reduced the scale to one

chaldron; so that your readers may be better able to form a judgment of the differences. In the Chartered Company, the whole rental is 125,977*l.*; the number of chaldrons of coals carbonised 20,678, giving a rental upon each chaldron of 6*l.* 1*s.* 10*d.*

In the City of London Company, the whole rental is 30,839*l.* the number of chaldrons of coals used 8,840, yielding a rental upon each chaldron of 3*l.* 10*s.*

In the South London, the rental is 14,963*l.*; the quantity of coals 3,640 chaldrons, producing a rental on each of 4*l.* 2*s.* 3*d.*

The cost of each chaldron of coal after deducting the profit on the coke, I reckon to be about 30*s.* The tar and ammonia may be considered an equivalent for the expence of lime. This will be the same with each Company.

The profit I calculate by the dividend which is paid on the capital advanced; in the Chartered Company the capital expended is 580,000*l.*; 8 per cent. on that, which is the amount of dividend, is 46,400*l.*: this is their profit. In the City of London Company, the dividend is 7 per cent. which, on a capital of 131,250*l.* gives for profit 10,240*l.*

In the South London Company, a dividend of 7½ per cent. on 96,000*l.* gives 7,200*l.*

The cost of coals and the profit being deducted from the whole rental leave the remainder for expences of management, wear and tear, labour, and contingencies. In the Chartered Company, the rental upon each chaldron of coals which is 6*l.* 1*s.* 10*d.* will be thus divided: cost of coals, 1*l.* 10*s.*; labour, management, &c. 2*l.* 7*s.* 11*d.*; profit, 2*l.* 3*s.* 11*d.*

In the City of London Company, the rental upon each chaldron of coals, which is 3*l.* 10*s.* will be thus divided: cost of coals, 1*l.* 10*s.*; labour, management, &c. 17*s.* 9*d.*; profit, 1*l.* 2*s.* 3*d.*

In the South London Company, the rental upon each chaldron of coals, which is 4*l.* 2*s.* 3*d.* will be thus divided: coals, 1*l.* 10*s.*; labour, &c. 12*s.* 9*d.*; prófit, 1*l.* 19*s.* 6*d.*

In the Chartered Company, the proportion of capital employed on each chaldron is 28*l.*

In the City of London Company only 14*l.* 16*s.*; and in the South London, 26*l.* 7*s.*

It must excite very considerable surprise to those at all conversant with Gas Companies, that such a very great disparity should exist in the statements given by these three Companies; first, in the great difference in the quantities of gas produced, and the equally great difference in the waste; next, in the vast disproportion of expence in management, that of the Chartered Company, with all the great advantages it possesses, being more than three times as much in proportion to the City of London Company, and nearly four times that of the South London. Again, though it has such a much larger proportion of lights upon the length of main; though it employs a much less number

of retorts for the number of lights, which may be seen by a reference to the table, yet it is stated to employ nearly double the capital to the quantity of coals decomposed in comparison with the City of London Company, and only a very little more than the South London; and notwithstanding all which the amount of dividend varies but very little, and the premiums on the shares are nearly the same in all.

Without any design, the managers of a Company may often times be mistaken in estimating their profits, more particularly when their funds exceed the capital employed, as many expences are charged to sunk capital which more particularly belong to wear and tear, &c.; for it is difficult to conceive that a gas establishment, like that of the City of London, with the wear and tear of 170 retorts, the average number in use—with the labour necessary for working them—with the other expences in management—of clerks—superintendants—inspectors—collectors—directors, for, I believe, there are no gratuitous services—law expences, &c. &c. should not expend above 7,839*l.*; and it is still more difficult to believe that the South London could effect all this for 2,302*l.*; while the Chartered Company is expending 49,060*l.* Yet it is upon such documents as these that Sir W. Congreve proposes to found his restrictive enactments; to regulate the price at which gas ought to be charged; and to do away with competition. For some years past, the most enlightened part of our legislature have been using their strenuous endeavours to do away with the evils that have arisen from over legislation; and here Sir W. C. wishes to submit the Gas Companies to an infliction of all those evils; but, we trust, that Parliament at this present day is too well informed to attend to such suggestions. He proposes that no competition should be allowed, and that the mains of each Company should be restricted to particular districts, that one may not interfere with the other; and to prevent any evil resulting from such a procedure, he further suggests that the price of gas furnished by the Companies should be fixed independent of their controul; that liable to all contingencies of increased expenditure, *they* of course are not to be allowed to make an increased charge; that is to be left to some other direction. And how would the public be benefitted by this? They may be secured against an increase of price by legislative enactment instead of competition, but what security have they against a deteriorated article? against a scanty supply? a diminished time of burning? or a slovenly and careless mode of supplying it? for gas may be adulterated, and its illuminating powers diminished by various methods; the pressure on the gasometer may be diminished, the mains may be supplied for a less number of hours, and less care may be taken in the purification of the coal gas. The Company is secure from competition, and it may remunerate itself by such means for the restriction it lies under. The reason which Sir W.

Congreve assigns for doing away with competition is, because in certain districts, the mains of different Companies now cross each other, and when there is a leakage, the parties are unwilling to be the first to open the ground, each being desirous of throwing the trouble and expence upon the other; but is this likely to be the case? Would the manager of a Company whose business it is to watch over its interests, knowing that a valuable article was escaping which might be at the expence of his Company, hesitate a single moment ascertaining the fact, and that merely because it might be the loss of some rival establishment? I can only say if the manager of a Company over which I had any controul acted thus, he would not continue to fill that situation long. It is the interest of every Company that there should be no waste, and that interest will make them careful that there is no annoyance from leakage. Sir W. Congreve throws out a hint whether it may not be advisable to place Gas Companies under some licence, but would this measure be attended with any good result? Let the public be secured by such legislative enactments as Parliament may think fit against any possible danger that may arise, but do not let the Companies be fettered by licences, visitations, and other vexatious restrictions, which can answer no good end whatever, and will only tend to drive from the superintendence men of talent and respectability. If it be deemed advisable that an inspector be appointed to ascertain that the public are incurring no risks, let his powers be strictly defined; let him have no controul over the management, or any thing in which the safety of the public is not concerned; if he observes that they are risking that, let him remonstrate, and if not attended to, let him report to the higher powers, who will compel attention; that is all which the public have a right to expect from Gas Companies more than from any other institution.

Sir W. Congreve has given the result of some very interesting experiments on the explosive force of coal gas mixed with atmospheric air compared with gunpowder; surely he will not draw a comparison between the danger arising from the two. It is not enough to consider because 39,000 cubic feet of carburetted hydrogen mixed with four times its quantity of common air will explode with the same force as 135 barrels of gunpowder, that, therefore, the vicinity of one is as dangerous as the other: we are also to consider by what means their danger is called into action. Gunpowder is already in its explosive state, and a spark dropped among a few loose grains scattered about where there are several barrels filled with it, would most probably explode the whole; but what a combination of circumstances must exist to produce the same effect with a gasometer filled with carburetted hydrogen. In that state it is perfectly harmless; a candle may be taken into a gasometer-house with impunity, and no one would dream of any danger arising from it. If there should

be an escape, and a candle allowed to approach it, the gas would ignite, and burn like a gas light, and would be as readily extinguished. Long before an escape of gas would become of such magnitude as to be dangerous from its admixture with atmospheric air, the smell would have given such ample warning, that some method would be adopted for preventing its continuance. If a gasometer were to turn on one side, there would be but a partial escape, and even if it took place in a building in the vicinity of the Retort House, from the levity of the gas, it would have a tendency to make its way through the upper part of the building, and would be hardly disengaged in such quantities as to form an explosive mixture that could reach the retorts. If a gasometer were to burst, still the escape would be gradual, and there must be a combination of extraordinary circumstances in this as in the former instance, before explosion could take place: neither would lightning have any effect on a full gasometer. I can conceive that if a gasometer filled with a certain portion of carburetted hydrogen and air so as to form an explosive mixture, were suddenly to burst in the vicinity of fire, that explosion would take place; but I find it very difficult indeed to conceive, how even a very large escape of unmixed carburetted hydrogen should become of such magnitude, and remain so confined, as to render all the air in the gasometer-house, in the retort-house, &c. buildings of no very limited extent, explosive, to me it appears almost impossible. Sir W. Congreve also contemplates an escape in an unfrequented building, such as a church, or meeting-house, &c. which may become dangerous. This has been so ably and so amply considered by Mr. Brande, some few years ago, that it is quite unnecessary for me to say any thing upon that subject: he expresses too some apprehensions from the breaking of the chain of the gasometer, which, by enlarging the flame of each lamp, might occasion fire. I should be inclined to think that the sudden increase of pressure would rather tend to extinguish the lights: at all events, the increase of flame would sufficiently inform persons of their danger, which might be readily removed by the turning of a cock.

It is a matter of surprise to me, and no doubt is so to many others, that a gentleman who is identified with explosions, whose name, as the inventor of one of the most powerful explosive engines, is known all over the world; who is more familiarised with that subject, and who has had more to do with it than any other person, should express what to me appears so many groundless fears on the present occasion. It would be wrong for the encouragement of any improvement in science, however great, to shut our eyes to the dangers of it; but it is, I think, still more impolitic to excite useless alarm, and apprehend evils that do not exist. Excepting the accident at Woolwich, with the particulars of which I am wholly unacquainted, all the accidents which I have ever heard of have been trivial, and have arisen

from gas escaping in close confined places, under shop counters, in vaults, dry wells, and places of that description, where the explosion has been but trifling, and little mischief done. Indeed if the danger be at all adequate to what Sir W. Congreve has described, it is a matter of inconceivable surprise that so very few accidents should have occurred. It appears to me that more mischief is to be apprehended from the bursting of those tanks which stand out of the ground, and indeed when they were filled with coal tar, the most dreadful consequences would have ensued from their giving way: the latter risk is, however, happily removed. Sir W. Congreve's recommendations concerning the size of the gasometers, the limiting the number in a particular space, the constructing them in the open air, may be the very acme of prudence, but I should very much doubt their necessity, and I am very sure of the very great inconvenience and additional useless expence which their adoption would occasion. It might be a very prudent and effectual precaution for a person never to go on the water to secure himself from drowning; but there are few, I believe, who would not laugh at him for adopting it.

Sir W. Congreve gives a short account of the Oil Gas Works at Oldford, and speaks favourably of the adoption of oil for the purpose of gas lights. I was not aware that Sir W. Congreve had paid an official visit to those Works. Had I continued in the neighbourhood, I should have been most happy to have attended him on the occasion, and have afforded him every information he might have required. I think it necessary to make one or two corrections of the statement given in the Report. The capital advanced is 8000*l.* instead of 6000*l.* This of course includes every expenditure, law expences for obtaining the Act, meters, &c. &c. The charge for gas is stated to be 50*s.* per 1000 feet: from that, however, 5 per cent. has been deducted *on account of the price of oil,* so that the real charge is only 47*s.* 6*d.* instead of 50*s.* In drawing a comparison between the illuminating powers of oil and coal gas, he says it is about as one to three; that is, *that one oil gas lamp will give as much light as three of coal gas.* The difference between oil and coal gas is not estimated in that way, because it must be a very large lamp indeed that will consume an equal quantity of the former as the latter. The holes through which the oil gas passes are only the 60th part of an inch in diameter; while those of the coal gas are, I believe, the 30th part, being four times the area. Oil gas passing through a coal gas burner under the same pressure emits a great deal of smoke; and I have observed a very remarkable circumstance, which corroborates a former observation of Mr. P. Taylor, that in burning oil gas or coal gas through the large hole burners, more than double the quantity of the latter is consumed than the former. In the course of my experiments, I applied one of the common street

burners, fancifully termed a bat's-wing, to the gasometer of oil gas, with 7-8ths of an inch pressure. There was a very intense light, accompanied by a great deal of smoke, and the quantity consumed, which was accurately measured, scarcely exceeds two feet per hour; while in coal gas it burns at least five feet. The way in which the difference between oil and coal gas is estimated is, that in lamps giving equal light, the one will consume one and one-third foot per hour; while the other will burn five feet in the same time.

It is impossible from the statement given in the Report, to draw any correct comparison between the advantages of oil and coal gas; yet if we take the most favourable part of each Company, and compare it with oil gas, we shall find the superiority of the latter to be quite as great, if not greater, than I have before mentioned. The average quantity of gas consumed by each burner of the Chartered Company, allowing for waste, cannot be less than 5½ feet per hour. By an accurate account kept of the quantity of gas consumed each hour during the night for several nights at the Oldford Works, and taking the hours between two and four, the time when only the public lamps were alight, and the number could be correctly ascertained, the average quantity of gas for each lamp was from 1¼ to 1⅓ foot per hour, making it rather less than 1 to 4. This quantity is a pretty near average for the private lights. Comparing this with the other two Companies, it would be nearly as 1 to 5½: my experiments were always as 1 to 4; but I calculated only as 1 to 3½; in both I have been within the mark.

In the amount of capital I will draw a comparison with the City of London Company. This is very little more than half the Chartered, supposing their statement to be quite correct—that they are quite clear of debts—and that their whole expenditure has not exceeded the sum stated in the Report. To produce the same number of lights with oil gas, a sum of 15,000*l.* for all necessary erections, apparatus, &c. law charges, and other contingencies, exclusive of mains, would be ample for every expence. The cost of 50 miles of main would not exceed 35,000*l.* so that a capital of 50,000*l.* would be sufficient. This is somewhat more than one-third compared with the above-mentioned Company, but with the Chartered, and their statement carries the greatest appearance of correctness, the difference would be considerably more. From the comparative small capital which is required for an oil gas establishment, it is clear if the same profit be derived from every light, the amount of dividend upon the money advanced must be, taking the average, three or four times as much in the one instance as in the other.

There is also another great advantage attendant on oil gas establishments which I have not sufficiently dwelt upon, and that relates to the current expences. In the Chartered Company, the

cost for producing 12,000 feet of gas, supposing that quantity to be procured from a chaldron of coals, is *3l.* 17*s.* 5*d.* of which the cost of materials is 1*l.* 10*s.*; and the remaining 2*l.* 7*s.* 5*d.* is for labour, wear and tear, management, &c. To produce 3000 feet of oil gas, the cost of materials will be, oil being 25*l.* per ton (the Oldford Company have never yet paid more than 22*l.* including casks); 3*l.* for oil, and 5*l.* for coals; while the cost for labour, wear and tear, &c. would not exceed, taking the average of full and slack work, 4*s.* per 1000, or 12*s.* for the 3000 feet. The profit upon that quantity by the tables in the Report will be 2*l.* 3*s.* 11*d.*; and in the Oil Gas Company, allowing one-third of the quantity consumed to be for public lights, which pays about one-half, would produce the sum of 2*l.* 3*s.* Now whether there be a large or small demand for gas, the current expences do not vary much. The establishment must be kept up, and where there are many occasional lights, they must be always in readiness to supply them, if they should be wanted. The interest on the capital too always remains the same. In the Oil Gas Establishment, both the one and the other are comparatively very small, and the greater expence, the cost of materials, ceases when no gas is required; while in the other, the smaller expenditure, namely, the cost of materials, ceases; but the greater ones, the current expences, and the interest on capital, continue, whether a small or large quantity of gas is required. Thus in every way the advantages of oil gas are most clearly manifested.

I am aware that the foregoing observations will not afford any more satisfaction to the advocates for coal gas than my former statements have done. Whether that dissatisfaction has been expressed in any of the monthly journals, I have but few opportunities of knowing, as I seldom see them, though I understand it has been inserted in some of the provincial newspapers, accompanied by insinuations which it is not worth my while to notice. I state what appears to me to be facts. If I am incorrect, let me be proved so by direct argument, and the public, or that part of it who are interested in the subject, will judge between us.

Yours truly,

M. Ricardo.

Article II.

Essays on the Construction of Sea Harbours. By Mr. J. B. Longmire.

(*Continued from* vol. v. p. 182.)

(To the Editor of the *Annals of Philosophy.*)

SIR, *Troutbeck, May* 20, 1823.

On the following phenomena of fluids, in direct and reflected motions, depend the disposition of the piers; which is perfect, when the water in the harbour is still, or nearly still; the surf, as little increased as possible by the piers; and when they interfere not with the lines of approach.

a. Straight waves, driven by the wind, directly through an opening into the still water of an harbour, assume curved figures; which, as they advance, become nearly semicircular, increase in length, and decrease in depth, till they are quiescent. Waves obliquely driven through a given opening do not agitate the interior water so much as direct waves; for the length of the waves that can pass through decreases as the angles of obliquity increases.

The waves, in moving over the surface, give the water under them impulses that create an agitation, extending much beyond them. This, which is here designated lateral agitation, to distinguish it from the surf, acts in and near harbours through the whole depth of the water, and appears on the surface in flat and slow undulations.

If a part of the harbour be separated by an inner pier, which only leaves a small entrance, not far from the principal one; then small parts of the waves pass into this bason; but being previously much decreased in height and velocity, and the lateral agitation considerably weakened, they very little disturb the water in it.

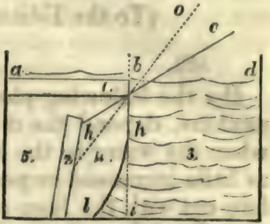
The surf, driven by a strong gale, through an entrance facing the sea, and sixty yards wide, provided the pier heads be opposite and parallel, requires a space equal to 1,200 square yards, to be quieted so much, that the rest of the bason have not undulations larger than one foot high.

When a gale commences, the waves but slightly disturb the interior water; yet by reiteration, the agitation greatly increases. Now the art of stilling the water in a harbour that admits the surf is to allow space for it to dissipate, and so to proportion an entrance to the area of an inner bason, that when the agitation is at its utmost, the undulations at the surface, where vessels lie, shall never exceed 15 inches, nor be repeated oftener than five

times in a minute. Any increase to this rate would injure vessels in the ebb tide, when they strike the ground.

b. The surf is raised, and driven forward by the wind, and of course takes its direction. If a part of the surf be stopped by the outside of the pier, the adjoining part continues to move forward, leaving the water on the innerside undisturbed, except by lateral agitation, for a given distance forward. Let fig. 3, represent this position. 1, 2, are parts of the piers of a harbour, rejecting the surf. Let *b*, 1, be the direction of the wind, and *a b d* a line of waves moving in this direction. The part *a b*, is stopped by the pier; but the part *b d* continues to move forward, say to *h*; beyond this point, the waves lengthen out towards the pier 2; and are limited on this side by the curved line *h l*. Thus while the exterior sea 3 is covered with a high surf, the part 4 has only the lateral agitation; and the harbour 5, being acted on by the same force only through the entrance *h* 1, is nearly quiescent.

Fig. 3.



The passing surf affects the harbour 5 least in the direction *a d*; as the sea within this line, in the parts 3 and 4, is comparatively smooth: and most when coming in the line 0,2; as then the part 4 exterior to the line 2,0 is equally covered with the part 3, by the surf; and of course a stronger lateral agitation is forced through the entrance. The surf indeed almost enters the harbour when driven in the line *c h*, and would disquiet it more than in either of the former courses; but that a gale in this line, making only a small angle with the lee shore, never raises a high surf at the harbour, and the surf is least in a gale directly into the entrance *h i*; for as it faces the calmest quarter, or, in other words, as the lee shore, at no great distance, projects into the sea, and covers the entrance, this gale comes over too small a range of sea, to force an injurious surf into the harbour. This description developes the mode of forming a harbour that rejects the surf.

c. When a wave strikes a pier at right angles, it rebounds directly back; but if it strike obliquely, the angle of rebound is equal to that of percussion. In oblique percussion, the reflected surf is greater at the leeward than at the windward end of the pier, by the amount of such surf collected through the whole, or a part, of the length, according to the strength or frequency of the impinging waves. The surf that strikes the pier at an angle of about 25°, sends the greatest quantity of reverberated water to the leeward end; and that impinging under an angle of 45°, disturbs the sea in front, to the greatest distance forwards.

The terms leeward and windward ends are used relative to the

direction of the wind, and change at the same end as the wind passes from one into the other of the two quarters in front of the pier. This alternation in the direction of the surf makes it difficult to adjust the direction of the enclosing piers so as to prevent the strong surf in any gale from passing along such piers to, and accumulating at, the entrances.

I am, Sir, your very humble servant,

John B. Longmire.

(*To be continued.*)

Article III.

On the Temperature of Mines. By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy.*)

Dear Sir, *Helston, May* 11, 1823.

Nearly twelve months have now elapsed since the temperature of many parts of Huel Abraham, Crenver, and Oatfield Copper Mines, in this county, were taken, an account of which you did me the favour to publish in the *Annals* for January last. Many of the experiments were a few days since repeated in precisely the same spot, and under similar circumstances, as before, and nearly with the same results, excepting the temperature of the water accumulated at the bottom of Oatfield engine shaft below the depth of 182 fathoms from the surface, in consequence of the pumps being drawn up from below that level.

The coldest part of this water, ten months ago, at the depth of 1,164 feet from the surface, and in 12 fathoms of water, was 66°. Last week, at precisely the same depth, it was only 59°; while the water at the surface of this shaft was 77°. This increase of temperature at the surface is to be attributed to the immense quantity of warm water sent from distant parts of the other mines to this shaft to be drawn out; and although it falls several feet into this shaft, which keeps the water in a constant and great agitation, yet it does not effect the temperature at the above-mentioned depth so much as might be expected.

I regret much that the registering thermometer could not be sunk to a much greater depth, and quite out of the influence of the falling waters, as I am inclined to think that it must ere this have arrived, or nearly so, to the mean annual temperature, or 53°.

I have before shown that by admitting the gradual increase of temperature (according to our descent) after a certain ratio, the temperature of this depth ought to be, at the lowest calculation, 70°. How comes it then to be less by 11° and 18° *minus* since this place was in the full course of working?

I have also found that the temperature of a working spot in Huel Abraham, at the 180 fathom level, where the difference of atmospheric pressure was 0·964, or nearly one inch, when other circumstances, such as number of men, current, blasting of rocks, &c. &c. were similar, that the difference of temperature was only from $1\frac{1}{2}°$ to $2°$; it being 78° when the thermometer was lowest, and $79\frac{1}{2}°$ to 80° when highest.

If these remarks appear to contain any further necessary information respecting the temperature of our mines, in continuation with what has already appeared, your inserting them in the *Annals*, will much oblige your humble servant,

M. P. Moyle.

Article IV.

On the Transition Formation of Sweden. By Dr. Forchhammer.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

The curious facts respecting the transition formation of Norway, which were discovered nearly at the same time by two German geologists, MM. Von Buch and Hausmann, have excited a great degree of interest, and although much which was at first supposed to be peculiar to the mountains of Scandinavia, has been found in other countries; and much which was imagined to be an exception, appears now to lie within the rule; yet enough remains to distinguish this formation from all others, and to show that the chemical power, which acted so strongly in the formation of the primitive rocks of the north, exerted its influence equally on the transition formation. It ought not to be forgotten, that a long time before the geologists now mentioned made their discoveries, Hissinger had made known a number of facts on this formation, with regard to Sweden, and several writers of minor note in respect to Norway; but the most interesting had not been observed, and the rest had not been connected in such a way, as to give any precise idea about the relative age of these formations, so as to compare them with those of other countries. The German geologists found, that porphyry, syenite, granite, in the neighbourhood of Christiania, rested on limestone and slate, and, while the first rocks contained zircon, feldspar, hornblende, paranthine, epidote, beryl, molybden, the others contained the fossils of marine animals. With respect to Sweden, M. Hausmann has given some very interesting notices, principally about the transition trap of Westgothland, and the transition porphyry of Dalerne. A few years

ago, Dr. Wahlenberg, of the University of Upsala, celebrated for his travels in Lapland, his discoveries relating to the geography of plants, &c. gave an account of the extent of these formations in Sweden, which, though it mostly concerns their geographical connexion with the primitive formations, and the fossils imbedded in them, affords, nevertheless, a great deal of information. Two papers have appeared; the first on the geological formation of Sweden, printed in the first volume of a periodical work, called Svea; the second is a paper on some petrifactions, which has been communicated to the Society at Upsala, and though printed several years since, has not been published, and a few copies are only in the hands of the friends of the author.*

It is much to be regretted that the author has not imitated the above mentioned travellers, in stating what he owes to the labours of his predecessors; so that it is often difficult in his works to distinguish his own discoveries, and even his own observations from those of others. We are going to notice such of M. Wahlenberg's observations as appear new to us, and we shall add such facts from preceding observations as will be necessary for illustration. It is to be regretted that we are not able to do the same with regard to Norway; but except the transition formation round Christiania, very little is known. We may, however, expect much from the zeal and information of several travellers, who have been some years occupied in a thorough investigation of the geological nature of this extensive country.

Every thing in the transition formation of Scandinavia, the nature of its rocks, its position with respect to the primitive mountains, its geographical situation, bears a peculiar character. Rocks of every description are found in it, mostly distinguished by their crystalline structure. It was in Scandinavia that granite was first discovered to be a member of the transition formation. Syenite occurs likewise frequently, and of the numerous varieties of the trap family, the two extremes, crystalline greenstone on the one side, and basalt and amygdaloid on the other, have both been observed. Sandstone and quartzrock, granular and compact limestone, clayslate, siliceous slate, alumslate, and even beds of whetslate occur. The slate itself is frequently bituminous, so much so that it burns; and even thin beds of coal occur at Billingen, in Westgothland. The shale which contains much bitumen is free or mostly free from lime; it is then an excellent alumslate, and a number of alumworks are supplied with it. It is distinguished from the alumslate of other countries, and the bituminous shale which is used in the alum manufactories in Scotland, by containing, besides sulphur and alumina, a sufficient quantity of potash, so that nothing is

* Om Svenska Jordens Bildning af G. Wahlenberg i Svea. Tidskrift sor Vetenskap och Konst Foresta Hæftet. Upsala, 1818.
Petrificata Telluris Svevanæ examinata a G. Wahlenberg.

necessary but to burn the slate, and to allow it afterwards to remain sufficient time exposed to the atmospheric air, that the sulphate of peroxide of iron thus formed may be decomposed by the alumina and potash, and at last to dissolve the alum. This slate, when so bituminous as to burn, is used as fuel in the alum manufactories. Large round masses, of a pretty pure black limestone, highly impregnated with bitumen (swinestone, anthracolite, Werner) occur every where in this alumslate. Round balls of sulphuret of iron and sulphate of barytes are likewise not rare.

The sandstone of this transition formation is distinguished from that of most other countries by its composition which is similar to that of granite; felspar, and even mica, are necessary to its composition; quartz being always in the greatest quantity. The almost absolute want of all useful metals in the whole formation distinguishes it likewise from the transition formation of most countries, and when compared with the primitive mountains of Scandinavia, which almost every where contain rich iron ores, which have copper in abundance in some places, rich mines of cobalt and silver, and where even gold has been found on different places, such a deficiency of metals must certainly excite surprise. For in other countries the rocks, and principally the slate and limestone of the transition formation are as rich as the primitive rocks. In two places of the transition rocks in Scandinavia, attempts have been made to work mines of galena; one in Scaane, near Cimbrisham, and another in Norway, not far from Stroemsoe, but both have failed.

The primitive mountains when compared to those of the Alps, exhibit a very material difference, both in external appearance and composition. The mountain chain which separates Norway from Sweden does not, at its highest point, attain 8000 feet, but it surpasses on the other hand the primitive chain of the Alps both in length and breadth. Its rocks are mostly such as it would be difficult to say whether they are gneiss or granite. From the main ridge, numerous parallel ridges of the same rock extend to the Gulf of Bothnia and Baltic Sea, thus forming a number of valleys and plains, which begin at the coast, and terminate at the neighbourhood of the boundaries between Norway and Sweden. In these plains, most of which likewise consist of primitive rocks, the richest beds of magnetic iron ore are found, such as at Daunemora, Haesselkulla, &c.; but it is also in these valleys and plains, that the transition formation has had room to expand, with, however, this great difference from most others, that it contains many crystalline rocks. These rocks of the transition formation are confined comparatively to the lower places, with some remarkable exceptions however, and it is a very interesting fact, which we owe to the observations of Dr. Wahlenberg, that each of the greater lakes of Sweden has its transition formation, which extends in regular beds on the

shores. The regularity of the beds, together with the small angle of inclination in general, occasioned the slaty and calcareous rocks of this formation, which contain frequently a great number of fossils, to be considered as belonging altogether to the secondary rocks; while the crystalline sandstones or quartz rocks, the porphyries, the syenites, and granites, are, without hesitation, placed among those of the primitive class. The fossils, however, show sufficiently that these rocks have been formed early after the existence of organic life on the earth. "They are," says Dr. Wahlenberg, "mostly entomostracites (entomolithes Lin.) and orthoceratites, which both, more than any other petrifaction, differ from animals now existing, and prove their great age. Both are of considerable size, and thin, which evidently proves the perfect quietness of the medium in which they lived. Very remarkable are, in this respect, the entomostracites, frequently a foot or more long, and the cylindrical orthoceratites, amounting to two yards in length, which latter lie perfectly entire in the limestone. If we consider further, that a great number of the entomostracites had eyes, and that both they and the orthoceratites exist in very great number, we must be surprised at the powerful organisation with which nature began at once in the north." The ridges of primitive mountains, which spread out from the main ridge, separate of course all the different parts where the transition formation is found, and gives them the character so peculiar to Scandinavia, which is, that the transition formation forms a number of different *systems*, originally limited on all sides by primitive mountains, having, therefore, no immediate connexion with each other, and generally containing the same kinds of rocks, though often in a different order of superposition. One great exception of this law exists, however, in respect to three chains of mountains, that seem to spread from one point, and thus to be connected with each other. This point seems to lie in the main ridge itself. Helagsfjaellet, Svukkujaellet, are mountains composed of sandstone, situated in this main ridge to the east of Roraas, in Norway. From these one branch passes to the south of Norway; the great lake Mjoesen, which terminates on the west side of the Firth of Christiania, is partly bordered by transition rocks of this *system*. Another branch passes into Jemteland in Sweden; it seems to terminate at the Storrjoe (large lake) in this province. A third branch passes into the province of Dalerne, and terminates at the lake Siljan. It is extremely remarkable that all these branches have their beds of fossils only at that end which is furthest from the main ridge, when they reach the neighbourhood of the large lakes; and it is evident that the closest connexion exists between the fossils of the transition formation and these lakes.

Sandstone is the rock which is of all transition rocks most abundant in the main ridge. The mountain Svuddu, which, according to the measurement of Tillas, is 4422 feet high, and, according to the measurement of Hissinger, 4693 feet, is a conglomerate, which consists of the same materials as the rest of the Scandinavian transition sandstone; and Hausmann has identified it with those of other parts of the Scandinavian transition formation. The same author mentions an impression on the surface of a piece of sandstone which he found in the inn at Idre, where this sandstone is very frequent in the country around. It seemed to belong to the stem of a fernlike plant, such as are frequently found in the shale of the first coal formation. This is the only instance where fossils are mentioned to occur in in this sandstone; some doubt may, therefore, be entertained, whether it is not merely a lusus naturæ. If it really was an impression of a plant, it would be a direct proof that this sandstone or quartz rock belongs to the transition formation; while some geologists are nevertheless of opinion, that it is a member of the primitive class, principally because its position is frequently unconformable with the gneiss upon which it rests. This, however, does not prove much, because slate and limestone are, with few exceptions, conformable with the sandstone, and they contain numerous fossils. Besides, in some places not far from Christiania, in Norway, sandstone occurs even upon slate and limestone. Upon this sandstone and conglomerate of the mountain Svuddu, and a part of the main ridge in the neighbourhood, in the Swedish province of Dalerne an extensive porphyry formation rests, which furnishes the materials for the excellent works of art that are made at Elfdale, now the private property of the King of Sweden. The porphyry extends from the main ridge as far east as Mora; it is mostly of a red colour, the compact mass being either siliceous slate or hornstone, or compact feldspar. The sandstone passes distinctly into this porphyry. Breccia of bits of porphyry cemented together by compact feldspar and syenite likewise occur. The syenite is remarkable, as it furnishes a new analogy between the formations of Dalerne and the country round Christiania; it occurs near Aasbye, and contains zircon, which is so characteristic of the transition syenite of Norway. The beds of sandstone, porphyry, and syenite, where a distinct stratification may be observed are in general almost horizontal, the angles never exceeding 20°. On the north side of the Lake Siljan, a number of beds of limestone alternate with beds of granite, both in a nearly vertical position, and in a direction nearly north and south; their elevation above the lake amounts from 150 to 200 feet. On the south-east end of the lake, from Ickaan to the church of Rattwick, much granite is found with few and thin beds of limestone; near the church of Rattwick, a bed of limestone occurs, with grains of sand, and destitute of

fossils. Such beds exist only where no clayslate separates the sandstone from the limestone. Near Buda Chapel, a bed of sandstone is found between a bed of limestone and of granite; and on Osmundsberg (Osmundsmountain), a bed of clayslate joins them, containing graptolithes (a small kind of orthoceratites). It is curious that this mountain, where at least three beds of different rocks occur, is the richest in fossils of the whole province; and the uppermost bed contains, besides the common entomostracites, a great number of anomites, turbinites, and madrepora stellaris, with several remains resembling corals. Entomostracites, crassicauda,* and laticauda,† are peculiar to this mountain. The limestone is commonly red, like that from Gothland: now and then it becomes white: it then has no fossils, and sometimes contains galena. It is curious that no alumslate or swinestone (anthracolite, Werner), is found in this province accompanying the limestone, which, in other places, is generally the case.

The island of Gothland agrees with respect to the limestone and its fossils so completely with the Osmundsberg, that it must follow next. Gothland is by far the largest of all beds in Sweden, which contain petrifactions, so that it has almost as large a surface as all the rest together; which seems to be again in direct proportion to the greater basin in which it was formed, which is the Baltic. The transition formation is perfectly isolated, the nearest rocks of granite being at a distance of about fifty miles. It is, however, probable, that it rests on a flat plain of granite, partly covered by this formation, and partly by the sea, which is no where round the island of any considerable depth. The beds of limestone are perfectly horizontal, except at Thorsborg. Upon what kind of rock the limestone immediately rests is not known, except in one place in the west part of the island, where below it a calcareous sandstone has been observed, containing the same mytilites, as the Osmundsberg; and this proves still more the similarity between these two parts of the transition formation, so far distant from each other. No clayslate has been found in Gothland. The limestone is light grey, compact, and does not contain any fossils, except on the faces where two beds join; they are a kind of imperfect fossil, which resembles a phacites; and which is peculiar to Gothland. On the faces of the upper beds occur a quantity of unusually large encrinites, anomites, and millepora; and upon the uppermost face, a great number of corals, turbi-

* Entomostracites crassicauda Wahlenb. "oculis ad angulos superiores capitis convexi, cauda subtriangulari; marginibus involutis crassissimis." It is very rare to find complete specimens; but different parts, principally the tail, have been found frequently.

† Entomostracites laticauda Wahlenb.: oculis ad latus capitis convexissimis, cauda suborbiculari; limbo latissimo planissimo radiato integerrimo. It is always twice as large as the former; and it is, therefore, not improbable, that it is a fossil of an older animal of the same kind. It has not yet been found entire, but only head and tail, and never in any other rock than greyish-white limestone.

nites, &c. which might only be expected in the largest system of Swedish transition rocks, that had been formed in the sea itself.

The island of Oeland consists likewise only of transition rocks, no granite or gneiss having been formed there. The limestone is at the utmost 140 feet thick; it covers the whole island, except a few places on the west side, where other rocks are seen, that lie under the limestone, viz. lowermost near Aleklinta, a sandstone very compact and free from lime; then follows a bituminous shale with subordinate beds of swinestone without fossils. These beds of shale are, when compared with those of the other systems, very imperfect, except in one place, where they increase in thickness, change into alumslate with the usual small entomostracitæ* in the beds of swinestone, and with small anomites lenticularis.† The limestone is usually red, and contains many orthoceratites, and the common entomostracites expansus; ‡ with these exceptions it is quite free from the remains of marine animals.

The system of Westgothland is one of the most interesting on account of the nature of the rocks which compose it, and the external appearance of the country which it forms. The large plain of Westgothland is formed of common gneiss, which, in many places, rises into small hills, never high above the level of the lake Wineren, and disappearing near the hills of transition rocks; so that it seems as if these had been deposited upon a perfect plain. The gneiss likewise near the rocks of the transition formation is somewhat different, principally on the east part of the plain; it contains green earth, instead of mica, and its feldspar weathers very readily; such is the rock at Lugnaes. The rocks belonging to this system are, (beginning at the lowermost), sandstone, which rests upon the gneiss, and which in the east part of this district is first met with at a height of 318 feet above the level of the sea; its thickness amounts to 77 feet, and it contains no fossils. Upon this follows the alumslate, the lowermost beds being the purest; the upper ones are often only a bituminous slate; together about 78 feet thick. The next layer is limestone, 202 feet thick; it does not contain beds of other rocks; but the limestone itself varies in colour, hardness, and the fossils which it contains: the lower part is white, semicrystal-

* *Entomostracites gibbosus.*—Cœcus, capite antice truncato planiusculo fronte oblonga, jugoque dorsali gibboso, cauda triangulari utrinque bidentata. Wahlenb. Entomol. paradoxus β cantharidum Linn. Syst. Nat.

Entomostracites scarabæoides.—Cœcus, capite hemisphærico antice rotundato, fronte subovato antrorsum angustiore, cauda utrinque sinuato-tridentato. Wahlenb.

Entomostracites pisiformis.—Cœcus hemisphæricus marginatus; fronte teretiuscula. Wahlenb. Entomolithus paradoxus γ pisiformis Linn. Syst. Naturæ.

† *Anomites lenticularis.*—Clausus (nullo foramine nec hiatu) suborbicularis utrinque convexicus oculus radiatim undulatus.

‡ *Entomostracites expansus*: oculis frontalibus, capitali testa antrorsum semiorbiculari plana laevi; caudali magnitudinem capitis, &c. Wahlenb. Entomolith. paradoxus et expansus, Linn. Syst. Nat. Trilobites dilitatus, Brunnich; T. novus, Schlottheim.

line, and contains only echinosphaerites pomum. The next is grey; it contains large entomostracites and few orthoceratites; the uppermost is mostly red, and contains a great number of large orthoceratites. Upon the limestone follows a bed of clayslate, 122 feet thick, of which the lowermost part is like the bituminous slate of the bed below the limestone; so that it is frequently necessary to distinguish them by their fossils, of which a small kind of orthoceratites is peculiar to the bituminous slate above the limestone. Next follows a chertlike liver-coloured kind of stone which now and then forms beds of the thickness of a foot, and contains echinosphaerites aurantium; uppermost lies a whitish stone resembling sandstone. Upon these beds of slate rests a large bed of greenstone; it weathers readily, and and falls to sandy grains; on this account the people call it sandstone. It is often divided into four-sided columns perpendicular to the stratum upon which it rests.

These rocks, of which the transition formation of Westgothland is formed, occur in three places completely separated from each other; and it is highly probable that no connexion ever existed between them, because the relative thickness of these strata is different, and not a trace of transition rocks is seen between them, while it rises on these three hills to a very considerable height. The first and largest mass of transition rocks occurs near Falkjoping, where a large plain of sandstone extends from the sources of the river Lidaa to the mouth of the river Tidaa over nearly 30 miles. Upon this plain rest three similar plains of limestone, separated from each other by narrow valleys, and each of them containing two or three summits of trap. The first of these limestone plains called Storfalan (the large common), is remarkable for its fertility; it has two summits of trap, the Mosseberg and Aalleberg; the second limestone plain is Taaredalsberg; and the third, Billingen, almost entirely covered by the bed of trap. The alternation of hard and soft stone in these mountains, occasions the formation of terraces in all of them, and the whole trap family has received its name from the stair-like appearance of these hills; trapp in Swedish signifying *stair*.

Kinnekulle, a hill on the south side of the lake Weneren, consists of the same rocks, but the limestone is only 150 feet thick, and the summit of clayslate and trap rises 470 feet above the limestone; but it is impossible to ascertain how thick each of these two strata is. The whole thickness of the horizontal beds at the Kinnekulle is 730 feet. Halle and Kenneberg are two other hills of transition rocks at the mouth of the Gothaelf, near Wenersberg. The clayslate and alumslate are each only about 50 feet thick; the limestone seems to be altogether wanting, and the trap on the Hunneberg is 128 feet; on the Halleberg 166 feet thick. Peculiar to Westgothland are: entomostracites paradoxissimus, the largest of the whole tribe, which, according to some detached parts, must sometimes have been about a foot

in length, entire specimens of that size have, however, never been found; Entomostracites bucephalus, of which the head only has been found, but which seems hardly to have been inferior in size to the preceding. These occur in the alumslate, which, in this system, is more perfect than in any other; but besides these and the common small entomostracitæ and anomites lenticularis, no fossils have been found in this slate. The limestone contains the large orthoceratites, the common entomostracitæ, and echinosphaerites pomum; all coralline petrifications, and all anomiæ, are wanting, but these again occur in the sandstone-like slate immediately below the greenstone, at a height of about 800 feet above the sea. It is extremely remarkable that these fossils are only found in the uppermost beds; in Westgothland in this clayslate, in the island of Gothland in the uppermost bed of limestone.

The transition formation in Oestergothland is low, mostly covered with gravel and earth, and no interesting fact has been observed respecting it; the same is the case with that of Nerike; and in Upland, only a number of loose blocks have been found, but no transition rock *in situ*.

In Scaane, the southernmost province of Sweden, and on two sides bordered by the sea, this formation is of great extent, but so scattered and so much covered by beds of gravel and sand, that the connexion of its different parts is not readily discoverable. In the south part, a long ridge of hills appears; the rock is white, and consists of granular quartz; it is in fact a quartz rock; mica, however, is rather rare in it. At Gladson, near Cimbrisham, it contains veins of fluor and galena, the fluor being frequently crystallized in regular octahedrons, a form which is rather rare. Limestone, alumslate, bituminous slate, and clayslate, occur in many places; even greywacke and perpendicular veins of greenstone, often several miles in length, and not seldom twenty or thirty fathoms in width, occur frequently. When the slate is weathered, there remain ridges of steep barren hills, which rise to 50 or 60 feet above the surrounding fertile country. No fossils are peculiar to this system of the transition formation, except entomostracites spinulosus,* of which entire specimens have been found only in Scaane, though in Westgothland fragments of the same animal occur. The alumslate has been worked at Andrarum for more than a century to supply an alum manufactory, and at a depth of 400 feet, they had not yet passed through it.

* *Entomostracites spinulosus.*—Cæcus, capite late semilunari, angulis posticis spinulosis, fronte oblonga convexissima, cauda rotundata spinulis trunci postremis breviore.

Article V.

On the Presence of Muriatic Acid in the Air of the Atmosphere. From several papers by Hermstadt, Vogel, Pfaff, &c.

The Dutch chemists appear to have satisfactorily ascertained the presence of muriatic acid in atmospheric air under certain circumstances, and the same fact seems to have been discovered a second time within the last two or three years.

M. Hermstadt, of Berlin, in a treatise on the sea-baths of Doberan on the coast of Mecklenburgh, first adverted to some properties which seemed peculiar to the air collected over the sea, or in its neighbourhood; the most remarkable circumstance was, that water shaken with it, precipitated nitrate of silver; he did not state his opinion that this was occasioned by muriatic acid, but left it undecided. Upon the suggestion of M. Vogel, of Munich, while on a visit to M. Kruger, of Doberan, the latter made some experiments which proved that water distilled from solutions of most earthy and even metallic muriates, contains some muriatic acid. The experiments were the following:

An ounce of muriate of potash was put into a distilling apparatus with 30 ounces of distilled water; the solution was kept slowly boiling, and 10 ounces of water were condensed; three drops of a concentrated solution of nitromuriate of platina were added to three ounces of this water, and the solution was evaporated in a glass vessel until only about five drops remained. On cooling, a reddish yellow sediment was deposited, which was difficultly soluble in water.

Solution of nitrate of lead, when mixed with the water, instantly produced turbidness.

Solution of nitrate of silver produced a similar effect, but more readily.

Litmus paper was not changed by the water.

This experiment was repeated, excepting that muriate of magnesia was used instead of muriate of potash. When the distilled water was heated in a silver vessel, and a few drops of solution of carbonate of soda added to it, every drop produced turbidness, which instantly disappeared. When eight ounces of the distilled water were evaporated with some carbonate of soda until half an ounce remained, a small quantity of a white precipitate appeared, which, when sufficiently washed, dissolved in sulphuric acid with effervescence.

Solution of nitrate of silver rendered the distilled water turbid, and nitrate of lead much more so. Litmus paper remained unchanged.

When the experiment was repeated with muriate of soda, the

same tests showed that a much smaller quantity of muriatic acid was carried over than in the former experiments.

When water was distilled in a similar way over muriate of lime, the distilled water became considerably turbid with nitrate of lead; in the common temperature of the atmosphere, neither oxalate of ammonia, carbonate of ammonia, carbonate of soda, nor nitrate of silver, produced turbidness; but when the water was boiling hot, both oxalate of ammonia and nitrate of silver occasioned turbidness. When five grains of carbonate of soda were dissolved in five ounces of the distilled water and evaporated to half an ounce, no precipitate was observed. Litmus paper was not at all affected by the distilled water.

Water distilled over muriate of barytes was rendered very turbid by a solution of nitrate of lead; a solution of nitrate of silver produced only a turbidness when added to the boiling-hot water. Neither carbonate nor sulphate of soda rendered the water turbid, but when it was boiled with sulphate of soda, turbidness appeared on cooling. Litmus paper was not affected by the water.

In water distilled over muriate of ammonia, both nitrate of lead and of silver immediately occasioned turbidness and precipitation. The same effect took place after the distilled water had again been subjected to distillation. Three ounces of the distilled water when mixed with three drops of a concentrated solution of nitromuriate of platina, and evaporated until only five drops remained, left a reddish yellow precipitate, which was difficultly soluble in water. Litmus paper was not affected.

Although nitrate of lead might not in all these experiments be a test of muriatic acid, for which it seems M. Kruger had used it, yet nitrate of silver was, with few exceptions, acted upon as if muriatic acid had been present. Any doubts which might remain as to the accuracy of the result have been removed by M. Vogel,* who boiled an ounce of completely neutral muriate of magnesia in 12 ounces of distilled water with sufficient precautions to prevent any of the salt from being carried over mechanically. The vapours were made to pass through a very dilute solution of nitrate of silver, and rendered it turbid in a quarter of an hour. One part of the solution was kept in a glass covered with black paper, and did not assume any colour; the other was exposed to the rays of the sun, and became red in a few minutes. In another experiment, the vapours passed through tincture of litmus, which they did not redden, but made the colour rather darker.

The same result was obtained when a solution of pure muriate of soda was distilled, and sea-water from the Mediterranean which had been kept in a laboratory for nine years, produced similar effects. The precipitate, which in these different expe-

* Gilbert's Annalen, 1822, No. 11.

riments was obtained, had all the properties of muriate of silver.

An opinion had been entertained that the property of precipitating nitrate of silver might depend upon the presence of sulphuretted or phosphuretted hydrogen. To refute this opinion, M. Vogel boiled down eight ounces of sea-water to two ounces; then added six ounces of distilled water, and evaporated until only two ounces were left; added again six ounces of distilled water, distilled again; and repeated in this way the experiment four times constantly with the same effect upon a solution of nitrate of silver. According to the experiments of M. Vogel, every kind of water he could get in the kingdom of Bavaria, either procured from rivers, springs, or brooks, contained so much of a muriate that it gave a precipitate with a solution of nitrate of silver. M. Vogel draws from these experiments the conclusion, that the muriates to a certain degree are volatilized by steam, and that they exist in the state of neutral salts in the distilled water. The same experiments were afterwards repeated by M. Bertram, who found that water when distilled with sufficient care over muriate of lime, did not carry over any of the component parts of that salt, for neither oxalate of potash, nor nitrate of silver, produced any turbidness in the distilled water. But when a solution of muriate of magnesia was distilled in a similar way, a considerable quantity of free acid passed over, and principally towards the end of the distillation when the solution became more concentrated. Prof. Pfaff* also repeated the experiments on boiling sea-water with sufficient care, and allowed the vapour to pass through a solution of nitrate of silver. He discovered a double action, the partial formation of muriate of silver, and the deoxidation of a part of the oxide of silver by means of pure steam. The result of his interesting experiments is this: when the vapour of pure distilled water is made to pass through a solution of nitrate of silver, this solution assumes all the different shades between yellow and dark-brown, according to the concentration of the solution, and the length of time in which the steam has passed through it. The colour is not very observable before the solution of the nitrate of silver has acquired the temperature of boiling water; but when it has reached it, the colour increases rapidly. If several glasses are connected by tubes, and all successively raised by the steam passing through them to the boiling temperature, all assume the colour. Nitric acid destroys the colour of this solution of nitrate of silver; and while the steam is producing this effect upon the solution, oxygen is disengaged. When steam, in a similar way, is passed through a solution of gold, a beautiful blue liquid is produced like that which is obtained by adding oxalic acid to a solution of gold.

It seems thus to be proved pretty clearly that the steam acts

* Schweigger's Journal, 1822, ix.

in these cases by deoxidizing the salts of silver and gold. Neither muriate of platina, nor protonitrate or pernitrate of mercury, were acted upon by steam in a similar manner.

The observations of the Dutch chemists, which are scattered in a number of different dissertations, have been collected and again published by Dr. Driessen,* and they possess great interest. Prof. Driessen, of Groningen, made the first experiments in July, 1800, at Amsterdam, where he poured several ounces of pure water 500 times through a glass funnel from one vessel into the other. The water sometimes exhibited a slight trace of sulphuretted hydrogen, but it constantly threw down nitrate of silver, of a white colour. These experiments were made at different hours of the day, and in different heights above the ground, but constantly with the same result, if it had not rained for a considerable time. When, however, M. Craanen, who had been present at these first experiments, tried them again after rainy weather, he did not obtain any precipitate at all. In Groningen, he did not find any muriatic acid in the air, except once in 1802, when, after a long dry season, a thick fog came on; water which had been poured in the way above-mentioned, occasioned a precipitate in nitrate of silver, and reddened even tincture of litmus. Dr. Von Rossem could not afterwards detect any trace of muriatic acid.

The fact that the air near the sea-shore contained free muriatic acid was applied to explain the frequency of that dreadful disease the colica saturnina, at Amsterdam, where it had been observed oftener than in any other town. It was conceived that the free muriatic acid dissolved the lead from the roofs of houses, and communicated it to the rain water.

A new series of experiments was, therefore, performed by Dr. Veehof in order to ascertain whether the muriatic acid was really in an uncombined state in the atmosphere; and the results were, that water poured from vessel to vessel at Groningen in the manner already mentioned, and rain-water from the same place, contained no free acid; that water similarly treated near salt springs showed a slight trace of free acid; and lastly, that water at Amsterdam, under the same circumstances, contained a considerable quantity of uncombined acid.

These experiments were twice repeated, and constantly with the same result. They all showed muriatic acid by nitrate of silver, but that from Amsterdam most of it. Besides the water from Amsterdam produced a precipitate when tried with muriate of barytes, and caustic alcali occasioned a more copious precipitate in it than in water, treated in the same way at Groningen.

Prof. Driessen repeated his experiments in 1809 at the Zuider Zee, where, after having poured the water more than 1000 times from one vessel to another; while the direction

* Schweigger's New Journal, b. 6, 2. 1822.

of the wind was such as to carry the breath and perspiration of the surrounding persons away from the water, he found unquestionable traces of free acid; they repeated the experiment again on one of the high dykes near Harlingen with the same result. It was observed that the colour of the litmus paper was particularly affected, when in a dry season the sea was violently agitated.

In order to ascertain the cause of this phenomenon, Dr. Von Rossem tried an experiment by exposing a vessel full of fresh sea-water to the rays of the sun, and poured, during that time, water over it; he found in the water distinct traces of muriatic acid.

The experiments of M. Vogel and M. Kruger, which occasioned the experiments about the volatilisation of muriates mentioned before, were the following: In a balloon with two apertures, one above and one below, to make draught, a small vessel containing a solution of nitrate of silver was introduced, and the balloon placed in a covered bathing car, of which one window was open, while the wind generally blew from the land. When, after 21 days, the small vessel was taken out, some bluish-black flakes, and a white powder, were formed. The precipitate, after having been washed, was digested with nitric acid, which dissolved the black flakes, and left a white precipitate, which was muriate of silver.

M. Meisner tried the air at Halle not far from the brine springs, but did not find any muriatic acid.

From all these observations and experiments, the following are the results: that the air near the sea-shore (the Baltic, the German Ocean, and the Channel, the latter according to some observations of M. Vogel), contains generally muriatic acid; its quantity is increased by dry seasons, and ceases to exist in rainy weather.

Muriatic acid may be found in the atmosphere at a certain distance from the sea-shore, and it there depends upon similar circumstances as on the coast. It exists mostly combined in form of neutral muriates, and it is highly probable that by the action of air and atmospheric heat, the earthy and alkaline muriates are not decomposed. In most of the experiments, they passed over at the boiling temperature in the state of neutral salts. Where muriatic acid was most decidedly found in a free state at Amsterdam, it is evident that this at least was partly owing to the sulphuric acid formed by the combustion of coal and peat.

Article VI.

An improved Method of making Coffee. By J. Smithson, Esq. F. R. S.

(To the Editor of the *Annals of Philosophy.*)

SIR, *June* 4, 1823.

From the highly fugacious nature of that part of coffee on which its fine flavour depends, a practice has become very generally adopted of late years of preparing the liquor by mere percolation.

This method has not only the great defect of being excessively wasteful, but the coffee is likewise apt to be cold.

Coction and the preservation of the fragrant matter are, however, not inconsistent. The union of these advantages is attainable by performing the operation in a close vessel. To obviate the production of vapour, by which the vessel would be ruptured, the boiling temperature must be obtained in a water-bath.

In my experiments I made use of a glass phial closed with a cork, at first left loose to allow the exit of the air. Cold water was put to the coffee.

This process is equally applicable to tea.

Perhaps it may also be employed advantageously in the boiling of hops, during which, I understand, that a material portion of their aroma is dissipated; as likewise possibly for making certain medical decoctions.

This way of preparing coffee and tea presents various advantages. It is productive of a very considerable economy, since by allowing of any continuance of the coction without the least injury to the goodness, all the soluble matter may be extracted, and consequently a proportionate less quantity of them becomes required. By allowing the coffee to cool in the closed vessel, it may be filtered through paper, then returned into the closed vessel, and heated again, and thus had of the most perfect clearness without any foreign addition to it, by which coffee is impaired. The liquors may be kept for any length of time at a boiling heat, in private families, coffee-houses, &c. so as to be ready at the very instant called for.

It will likewise prove of no small conveniency to travellers who have neither kettle, nor coffee-pot, nor tea-pot, in places where these articles are not to be procured, as a bottle will supply them.

In all cases means of economy tend to augment and diffuse comforts and happiness. They bring within the reach of the many what wasteful proceedings confine to the few. By

diminishing expenditure on one article, they allow of some other enjoyment which was before unattainable. A reduction on quantity permits indulgence in superior quality. In the present instance, the importance of economy is particularly great, since it is applied to matters of high price, which constitute one of the daily meals of a large portion of the population of the earth.

That in cookery also, the power of subjecting for an indefinite duration to a boiling heat, without the slightest dependition of volatile matter, will admit of beneficial application, is unquestionable.

Article VII.

On Ultramarine, and the Methods by which its Purity may be ascertained. By R. Phillips, FRS. L. and E.

Before the time of Margraff, whose analysis of lapis lazuli was published in 1768, the colouring matter of this mineral was supposed to be copper; according to the chemist just mentioned, as quoted in Klaproth's analyses, vol. i. p. 163, lapis lazuli consists of oxide of iron, silica, lime, and its sulphate, omitting any notice of the alumina which it contains in very considerable quantity, and without stating the proportions of the ingredients enumerated as its constituents.

Rinmann and Cronstadt have also mentioned the composition of this mineral, but their statements are so inaccurate as to require no further notice. According to Klaproth, lapis lazuli consists of

Silica	46·0
Alumina	14·5
Carbonate of lime	28·0
Sulphate of lime	6·5
Oxide of iron	3·0
Water	2·0
	100·0

With respect to the colour of this substance, Klaproth observes, that "though the researches of Margraff have refuted the opinion formerly received, that the blue colour of the lapis lazuli originated from an admixture of copper; and though it has been demonstrated that the colour of this fossil is owing only to iron, yet its other constituent parts have not yet been determined with due accuracy."

Now as neither the protoxide nor peroxide of iron could be suspected of imparting a blue colour, it is singular that Klaproth

should not have alluded to this circumstance, and have suggested the nature of the combination by which iron or its oxides might, with the other constituents, produce the blue colour in question.

The analysis of MM. Clement and Desormes, (Annales de Chimie, t. 57, p. 317), shows, that although lapis lazuli may yield oxide of iron on account of the pyrites it contains, yet ultramarine prepared from it is perfectly free from any; and before I was aware that they had determined this point, I had arrived at the same conclusion, and have repeated many of their experiments; and, as far as I have gone, my results and theirs agree.

According to the chemists just quoted, the colouring matter of ultramarine is not destroyed by a moderately strong red heat, remains unchanged by ammonia, and when heated in solutions of potash and soda. Acids, however, destroy the colour in a few minutes, and this effect is produced even by acetic acid, as well as by the nitric, muriatic, and sulphuric. They also state, and correctly, that solution of sulphuretted hydrogen has no effect upon the colour. According to their analysis, ultramarine consists of

Silica	35·8
Alumina	34·8
Soda	23·2
Sulphur	3·1
Carbonate of lime	3·1
	100·0

It is remarkable that MM. Clement and Desormes have offered no conjecture as to the nature of the colouring matter; and it was the wish to ascertain this that first induced me to turn my attention to it.

Although I have been totally unsuccessful in attaining the object of my pursuit, yet I have thought it might not be useless to state the experiments which I have made, more especially as the colour is extremely dear, therefore likely to be adulterated; and I am enabled to point out ready methods of determining its purity, and detecting the nature of any fraudulent admixture.

I am inclined to believe from the results of the experiments of MM. Clement and Desormes, as well as my own, that the colouring matter of ultramarine is a peculiar substance. I must, however, repeat, that I have obtained no direct proof of it. M. Thenard, alluding to the analysis of MM. Clement and Desormes, observes (Traité de Chimie, t. ii. p. 205), " Comme ils ont eu, dans cette analyse, une perte de 0·8, il faut en conclure que quelques principes leur ont nécessairement échappés. Ces principes ne joueraient-ils pas un rôle remarquable dans la coloration du lazulite? Cette opinion paraîtra probable, si

l'on considère que toutes les autres pierres doivent leur couleur à une matière colorante. On pourrait soutenir, à la vérité, que la silice, l'alumine, la chaux, la soude, quoiqu' incolores, sont susceptibles de former un composé coloré ; mais il faut avouer qu'il serait fort extraordinaire qu'il n'y eût qu'un composé de ce genre parmi ces pierres; et cependant c'est à cette conséquence qu'on serait conduit en admettant qu'il n'existe point de principe colorant particulier dans le lazulite : aussi M. Vauquelin croit-il que cette pierre contient de l'oxide de fer."

Although in the 34th vol. of the Annales de Chimie, Guyton also attributes the colour of ultramarine to iron, I need hardly again state, that ultramarine contains no oxide of iron, and, therefore, the opinion of the last-mentioned chemists, although meriting the highest attention, cannot be considered as well founded. Indeed the lapis lazuli examined by Klaproth contained only 3 per cent. of oxide of iron, and this, supposing it capable of affording a blue colour, could hardly be admitted to yield the intense blue of the lapis lazuli.

When any coloured earthy substance occurs, the first and most natural supposition is, that the colour is owing to the presence of a metallic oxide. There is however great difficulty in admitting this colouring matter to be a metallic oxide; for when it is destroyed by an acid, we may suppose one of several cases to happen, first, that the loss of colour is the result of the mere act of solution, as when we obtain a colourless solution by dissolving peroxide of mercury in nitric or muriatic acid: this, however, can hardly be the case with the colouring matter of ultramarine; for we do not by the addition of potash reproduce a blue substance; whereas from pernitrate of mercury, the oxide is precipitated possessing its original colour.

It may be supposed that the solution of ultramarine in acid is attended with the evolution of oxygen, and consequent loss of colour; but in this case one of three things would happen; first, that oxygen would be evolved in the state of gas, as when peroxide of manganese is heated in sulphuric acid; secondly, that carbonic acid would be formed and evolved with effervescence, as when peroxide of manganese is decomposed and dissolved by binoxalate of potash; or, thirdly, that when put into muriatic acid, chlorine would be evolved; the fact, however, is, that no one of these circumstances occurs.

On the other hand, it is possible that the peculiar colouring matter of the ultramarine may acquire oxygen during solution, and thus lose its usual appearance; to this, however, there is one experiment in direct opposition; viz. that sulphurous acid which readily absorbs oxygen, but does not impart it, destroys the colour of ultramarine as completely as nitric acid, which might be supposed to oxidize it.

When nitric acid is added to ultramarine, the colour is quickly destroyed, and a slight smell of sulphuretted hydrogen is

perceptible; it might, therefore, be supposed that the colouring matter is the sulphuret of some peculiar metal. To try whether the colour could, upon this supposition, be reproduced, I added sulphuretted hydrogen both to the solution and the colourless residuum, but no restoration of colour was effected by this or any other mode which I could devise. The only remaining supposition with respect to the metallic nature of this colour to which I shall allude, is the possibility that it may be in the metallic state. This, however, can hardly be the case, for if the colour be lost by oxidation, then when acetic acid produces the effect, hydrogen must be evolved from the decomposition of water; but this does not occur.

Although it is possible, as M. Thenard has stated, that colourless bodies may, by combining, form a coloured compound, I confess I rather incline to the opinion, that lapis lazuli owes its colour to a peculiar non-metallic substance; and I recommend the subject as worthy of the attention of chemists.

I shall now briefly state the methods of detecting various substances, which may possibly be employed for adulterating ultramarine.

Although we may almost venture to pronounce ultramarine to be genuine, which, in a few minutes, loses its colour when put into an acid, leaving insoluble matter of a dirty-white colour, and affording a colourless solution, I shall nevertheless mention certain bodies which it is probable may be mixed with ultramarine, and the methods by which they may be detected.

Blue Verditer.—If this carbonate of copper be mixed with ultramarine, it may be ascertained by heating the suspected colour on a piece of silver or platina foil in a spirit lamp. If there be any verditer present, the mixture will become almost immediately greenish, and eventually black. The degree of alteration of colour will of course depend upon that of the adulteration.

Genuine ultramarine loses its colour totally by being put into an acid, no effervescence is excited, a deposit remains of dirty-white colour, and a colourless solution is obtained which gives only a slight and colourless precipitate with ammonia; so that verditer, or any other cupreous compound, may also be detected by putting the colour into an acid. If a blue or green solution be obtained, and if ammonia added to it in excess gives a deep-blue solution, or if a drop of the acid solution leave a deposit of copper upon iron, it follows that some preparation of copper was present; and if the admixture of verditer be considerable, the evolution of carbonic acid will be evident.

Prussian Blue.—Genuine ultramarine suffers no change of colour by being heated, but if it contain prussian blue, its colour will be much darkened by heat; if also the genuine colour be boiled in a solution of potash, its intensity and brilliancy is rather increased than diminished; but if it contain any admixture of prussian blue, the colour will become browner, and the

solution, if not too strongly alkaline, will afford a blue precipitate when added to a solution of iron.

Indigo.—This substance when heated in a spirit lamp readily rises in the state of purple vapour. Sulphuric acid, even in its concentrated state, does not destroy its colour, and, therefore, the presence of indigo is very readily ascertained.

Smalts.—The colour of smalts resembles that of ultramarine in resisting the action of heat; but as it is not destroyed by any acid, and as the colouring matter of ultramarine is, any admixture of smalts will be easily discovered.

Colour prepared from Oxide of Cobalt and Alumina.—This compound, which greatly resembles ultramarine in appearance, although its colour is not quite so bright and intense, may be distinguished from it, by remaining unacted upon by acids precisely like smalts. Heat does not readily change its colour, but if a drop of solution of carbonate of potash be added to it on platina foil in the flame of a spirit-lamp, it is readily blackened; an effect which is not produced upon ultramarine.

Article VIII.

On the Geology of Devon and Cornwall.

By the Rev. J. J. Conybeare, MGS.

(*Continued from* vol. v. p. 184.)

Inferior Slate.—At and near its immediate contact with the granite appears as a somewhat indistinct and ill characterized gneiss, in some beds of which the felspar so predominates that they have been termed *slaty felspar*. Judging, however, from their less ready fusibility, from the large proportion of metallic oxides (iron and manganese) which they contain, and from the examination of many specimens from various quarters, we shall, I think, approach nearer to strict accuracy by regarding these as compact felspar intimately mixed with mica (or rather with chlorite) and quartz. To the geologist, who seeks the aid of mineralogy and of chemistry, examples of this intimate penetration of one simple mineral by another (so, as in many cases, to alter very considerably the external and empirical characters of that which yet predominates) must be familiar. Many subordinate beds of the earlier greenstone formation exhibit every stage of a similar phenomenon, and an accurate examination would probably show, that most of the substances named petrosilex, corneenne, saussurite, jade, and even flinty slate, are in fact only admixtures of this nature, in which felspar varying from its more compact and semitransparent to its earthy and granular form, is uniformly and intimately penetrated by some

variety or other of hornblende, of diallage, and occasionally perhaps of other minerals, which (as hypersthene) enter more sparingly into the composition of rock masses. Such admixtures can be properly studied only in those endless suites of specimens which nature herself preserves, and presents *in situ.* The subject is an interesting one, and well deserves closer attention and investigation than it has yet met with. The general character of the gneiss in question, the brownish purple colour by which it is almost every where distinguishable, its very limited extent, and its gradual passage into the common killas, have been sufficiently noticed by most recent observers. I do not recollect ever to have seen any mineral substances imbedded in its mass. It is of course *traversed,* like the adjacent rocks, by metallic and other veins. This passes, as is well known, by a rapid transition into the common *killas* or *clayslate.* Before entering on the consideration of this, by far the predominant form of stratified rock throughout the whole of the west, it may be well to notice two of its subordinate beds, or, perhaps, varieties, which, though much inferior in point of extent, yet present appearances much more clearly indicative of their mineral composition, and capable, perhaps, of throwing some light on that of the common *killas* with which they are so closely associated. 1. Common chlorite slate. This needs no further description, and I am not aware that it has ever been found to contain any other *imbedded* mineral than the *garnet,* specks of iron, and perhaps of copper pyrites. 2. A laminated rock, of a silvery grey colour, and micaceous aspect, exhibiting throughout its mass small patches of a darker tint, having the appearance of some imbedded mineral obscurely crystallized, and much intermixed with the slate containing it. These patches have been regarded as allied to grenatite, to hornblende, and to some other mineral species, but closer examination shows them (unless I be mistaken) to consist of a dark-grey chlorite minutely and confusedly crystallized. This variety of killas contains in the neighbourhood of Camelford, where it may perhaps be most advantageously studied, small contemporaneous veins of crystalline felspar; and in one or two instances alternates with thin beds of compact felspar tinged by the admixture probably of chlorite; a circumstance observable also in the killas of Wheal Maudlin; and in that which succeeds the granite near Ivy Bridge; though, in these latter cases, it is possible that the penetrating matter may be hornblende.*

But these varieties are, as I have stated, but of partial occurrence and limited extent. The stratified rock of the mining district is almost universally the common killas. This rock, after much question (which your readers would scarcely wish to

* I may here observe that the neighbourhood of Ivy Bridge offered by far the most beautiful and characteristic specimens of compact felspar unaltered apparently by any mixture, which I ever met with in the west.

be recapitulated), as to its being a variety of greywacke, which, if that term has any definite meaning, it unquestionably is not, has been at last admitted on all hands to be *genuine clayslate.* But this appellation perhaps, after all, does not convey a much clearer notion of the real nature and constitution of the rocks included under it than the repudiated greywacke. An opinion on this subject (nearly identical with that which has for many years been my own) is to be found in the very interesting Catalogue Mineralogique of the Comte de Bournon: " Les parties composantes qui entrent dans la substance du killas sont, le mica vert tres attenué nommée chlorite, le quartz, et le feldspath; et les varietes qu'il presente dependent de la manière dont ces trois parties se réunissent entr'elles." (Bournon Cat. Min. 463.) Mr. Hawkins, in a paper written evidently without the knowledge of C. Bournon's work (probably indeed from materials collected before its publication), appears to hold nearly the same view. " There is much reason (he writes) to consider it (*killas*) as an intimate mixture of quartz with mica, talc, chlorite, and perhaps, in some instances, with felspar. We may trace the last in those varieties of the slate which, in this country, are contiguous to the granite.* On the other hand, the talcose ingredient of this mixture is more conspicuous in the varieties which occur at a distance from that rock." If that, which I cannot but suspect to obtain as a general law (see *Annals*, vol. v. p. 189); namely, that stratified *rocks are in their mineralogical composition only varieties of the crystalline masses with which they are most largely and closely associated,* be admitted, we shall have difficulty in recognising in the numerous elvans by which it is traversed, the crystalline *analogue* of the killas. The substance occasionally termed claystone,† might perhaps afford a link in the series connecting the two extremes. At all events, that term, as well as clayslate, has been very vaguely applied, and is in itself ambiguous. A ready means of detecting the mineralogical constituents of these and the like obscure aggregates (if we admit them to be such), would be among the most valuable services which chemistry could render to geology.

It may be added that the inferior slate occasionally exhibits very remarkable instances of curvature and contortion. The coast of St. Agnes, a spot highly interesting both for the mineralogist and geologist, will afford more than one example.

* This restriction is not universally borne out by facts: it should rather have been stated, that " we may trace the last *more abundantly.*" The passages connected with, and following those which I have adduced, well deserve the attention of the geologist. There are some statements in the preceding half of the same paper which Mr. Hawkins himself, on recurring to the advances made in geological science since the period at which his materials appear chiefly to have been collected, would probably be the first to cancel or to modify. See especially p. 6.

† A rock of this character is found associated with the chloritic, though it seems more common in the amphibolic series.

These, however, are by no means so numerous or striking as those afforded by the greywacke of North Devon, a circumstance apparently adverse to the theory which would attribute these singular configurations to the agency of heat; for we might certainly expect that the killas, which is easily affected by that agent, near as it is to the central granite, and traversed in all directions by various dykes and veins, would have exhibited more frequent traces of this nature than the refractory and unbending sandstones. But this is a question of mere hypothesis. This portion of the *inferior slate* does not (so far as my knowledge extends) contain any *imbedded* minerals; near Camelford, and at some other spots, I have observed in it small contemporaneous veins or nests (vugs, as they are provincially termed) of crystallized felspar and chlorite. Most of its varieties are readily fusible.

(*To be continued.*)

ARTICLE IX.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

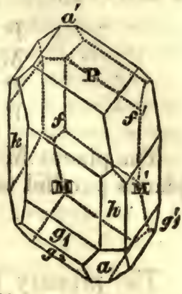
(*Continued from* vol. v. p. 452.)

IN my last communication I noticed the irregularity that frequently occurs in the forms of crystals, whether natural or produced by art, occasioned by an enlargement of some of the planes, and a consequent comparative diminution of others. This irregular character may be said to be almost general, and very frequently might lead to an erroneous determination of the true forms of crystals, if we do not attend sufficiently to the positions of their planes, to their cleavages, and to the measurements of their angles. Another circumstance will also tend to mislead us with regard to the forms of crystals, when compared with the drawings by which they are represented: this is the manner of their attachment to the mass to which they are united; sometimes they are attached by a *lateral* edge or plane of the figure exhibited in the drawing, and sometimes by the *upper* summit; in which latter case, the crystal would appear to be inverted, and the order of the lateral planes of several of the classes of prisms, when observed from left to right, would be reversed.

The measurement of corresponding planes on different crystals will frequently differ more than half a degree, and may occasion a difficulty in determining particular planes by measurement, when they meet at nearly the same angle. The angles given here are generally the mean of a considerable number of measurements.

Acetate of Soda.

The primary form deduced from cleavage is *an oblique rhombic prism,* the cleavages being parallel to the planes P, M, and M′, of the annexed figure. Some or all of the secondary planes on that figure occur on many of the crystals. On some crystals only the planes *k,* or *k* and *f,* accompany the primary planes, and on others only the planes *a* and *g,* with the addition sometimes of the planes *h.*



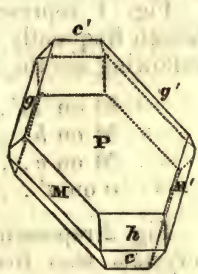
All the planes except *f* have been measured by the reflective goniometer.

P on M or M′	104°	25′
P on *f*	136	nearly
P on *a′*	103	35
M on M′	84	30
M on *k*	137	45
M on *h*	132	15
M on g_1	156	54
M on g_2	135	50

Acetate of Zinc.

The crystals are very thin, flexible, and soft, and fissile parallel to P, but do not afford any other measurable cleavage planes.

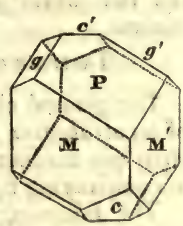
The primary form indicated by the natural planes of the crystals is *an oblique rhombic prism,* measuring as follows:



P on M, or M′	112°	28′
M on M′	67	24
P on *h*	133	30
P on *c*	100	00
P on *c′*	80	00
P on g, or *g′*	75	30

Binacetate of Copper.

The primary form developed by cleavage is *an oblique rhombic prism,* the cleavages being parallel to the planes P, M, and M′, of the subjoined figure; the secondary planes *c* and *g* are the only ones I have observed on the crystals, which are sometimes produced in pairs, and united by the planes *c,* in such a manner as to exhibit a second entire plane P, joined by an acute angle to the lower acute angle of that which



is exhibited in front of the figure, but inverted in its position so as to be terminated at its lower extremity by the planes *g* and *c*.

P on M, or M′........	105°	30′
M on M′.............	72	00
P on *c*′...............	119	4
P on *g*, or *g*′..........	131	45

The planes M and M′ are generally curved, and the cleavage planes parallel to these partake also of the same character.

Sulphate of Magnesia.

The primary form of this substance has been given by the Abbé Haüy as a *right prism* with a *square base*. But from the measurement of several crystals, and from the character of the secondary forms of some of those, the primary may be regarded as a *right prism* with a *rhombic base*, whose angles are 90° 30′ and 89° 30′.

I have found only one cleavage, which is parallel to the short diagonal of the prism, and consequently to the plane *h* of the accompanying figures.

Fig. 1.

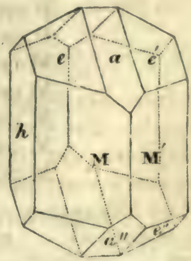


Fig. 1 represents a crystal of a form which frequently occurs, and of which the following are the measurements:

M on M′........	90°	30′
M on *h*.........	134	45
M on *e*.........	129	00
a on *a*′.........	120	nearly

Fig. 2.

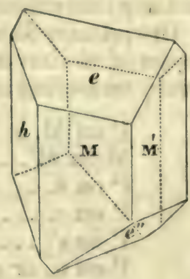


Fig. 2 represents a form under which the crystals also frequently appear. In this form, only two of the four planes *e* are seen on each summit, and alternating in position as shown in the figure.

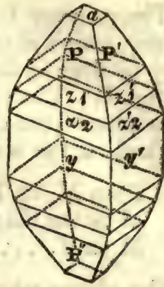
On some of the crystals, however, which resemble this figure, the two other planes *e* may be perceived, but they are very minute.

Tartrate of Potash and Antimony—Emetic Tartar.

The general character of the crystals of this compound is that of an *octahedron with a rhombic base*. I cannot discover more than one distinct cleavage, which is parallel to the plane *a* of the accompanying figure.

The following are the nearest to coinciding measurements taken on several crystals:

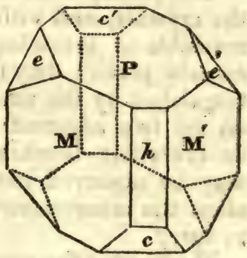
P on P′	108°	16′
P over the edge on the left.	104	15
P on z_1	166	40
P on z_2	165	40 nearly
a on P, or P′.	122	00
a on *y*	90	00



The planes *z* and *y* are generally striated, and afford imperfect reflections; and the crystals are frequently elongated in the direction of one of the edges of the base, so that the plane P terminates in an *edge* instead of a *point*, an irregularity of figure common to all the classes of octahedrons.

Sulphate of Potash and Magnesia.

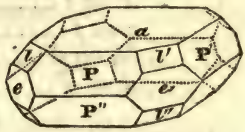
I have not found any cleavage of these crystals, but the predominating form, and which may be regarded as the primary, is *an oblique rhombic prism*, modified by the planes *c*, *e*, and *h*, and measuring as follows:



L on M, or M′	102°	20′
M on M′.	108	45
P on *c*′	116	45
P on *e*, or *e*′	154	30
P on *h*.	105	8

Ferroprussiate of Potash.

The crystals are soft, flexible, and very fissile parallel to the plane P of the annexed figure, and there is not any distinct cleavage that I have been able to perceive in any other direction. There are, however, in some crystals, apparent natural joints parallel to the planes P of this figure; these would give *an octahedron* for the primary form, which, from the angles of the secondary planes, is found to have a square base. The most distinct measurements are the

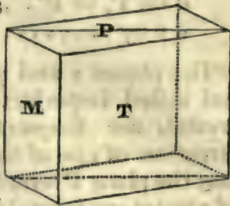


P on P″	137°	00′
P or P′ on *a*	111	30
a on *l*.	119	9
a on *e*.	90	0
e on *e*′.	90	0

Bicarbonate of Potash.

The primary form of this substance is *a right oblique-angled prism,* which is not readily traced in the secondary crystals, but may be derived from cleavage, and is shown in fig. 1. There is also a cleavage parallel to a plane passing through the diagonals marked on the terminal planes.

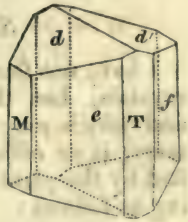
Fig. 1.



P on M, or T	90°	00′
M on the diagonal plane	53	15
M on T.	103	25

The planes which appear on the crystals are represented in fig. 2; but the planes *e* are sometimes very disproportionately extended, so as nearly to efface T and *f*, giving to the crystals the character of another primary form.

Fig. 2.



The planes T do not commonly occur on the crystals, and without these they nearly resemble a secondary form of the *right rhombic prism;* they may, however, be distinguished by the unequal inclination of M on the two adjacent planes. On cleaving or otherwise breaking the crystal, water may be observed between the laminæ, which probably occasions the measurements on the cleavage planes not accurately to agree.

This is also the case with many other of the factitious salts.

M on plane parallel to *f*	127°	35′
M on *e*	126	45
T on *e*	156	50
T on *f*.	128	50
e on *f*	105	40
M on *d*	111	00
d on *d′*.	138	00

Cyanuret of Mercury.

I have received for examination from Mr. Cooper, of Lambeth, some crystals obtained from oil of bitter almonds by digesting it with red oxide of mercury.

Mr. C. has also supplied me with some crystals of cyanuret of mercury, procured in the ordinary way by boiling the red oxide with prussian blue. The crystals derived from both of these sources correspond perfectly in their crystalline forms.

I have not succeeded in cleaving them, but from their measurements and modifications, a right square prism may be regarded as the primary form.

Fig. 1.

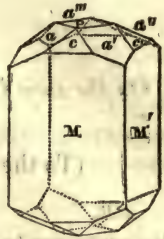


Fig. 2.

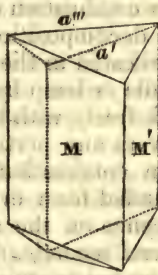


Fig. 1 represents the prism with the modifying planes which I have observed on two or three crystals, and on these only, out of a considerable number that I have examined.

Their general form is that shown in fig. 2, in which two of the planes *a* alternately efface all the other terminal planes at the two extremities of the prism. There are also many crystals which nearly resemble fig. 2, but in which the planes *a* and *a''* are visible, although very minute. This irregularity of form is of the same character as has been already noticed as belonging to sulphate of magnesia. The measured angles are as follows:

M on M'	90° 00'
c on M / *c'* on M'	132 45
a on M / *a'* on M, or M'	112 40
a' on *a'''*	114 00

Article X.

Astronomical Observations, 1823. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37' 44·3" North. Longitude West in time 1' 20·93".

May 18. Emersion of 69 Leonis from the moon . . . . 13h 41' 18" Siderial Time.

ARTICLE XI.

On the slow Combustion of Tallow, Fixed Oils, and Wax.
By Mr. C. J. B. Williams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Edinburgh, May* 1, 1823.

PERMIT me to draw the attention of your chemical readers to a phenomenon (hitherto, I believe, unnoticed), illustrating the slow combustion of the inflammable gas or vapour, produced by the decomposition of oleaginous matter by heat. It may be manifested in the following manner: Extinguish the flame of a candle or lamp by blowing on it, having previously supplied the wick freely with tallow to increase the size of the flame, and to prevent any portion of the wick from remaining in a state of open combustion. If this experiment be made in a room secluded from every other source of light, a distinct phosphorescence on the surface of the wick will be perceived during several seconds, brighter in proportion to the size of the flame before being extinguished; hence it is most obvious with a long wick, provided no spark be left on it.

This combustion, which I consider analogous to that of the vapour of ether, by the aid of a platinum wire, is likewise attended with the production of a pungent acid vapour. Of the nature of this, I have had neither time nor opportunity to ascertain any thing. It is probably, as in the case just alluded to, merely a modification of the acetic acid; but since the odour differs considerably from that of the lampic acid, it might be of sufficient importance to merit an investigation.

I have ascertained that a similar phenomenon occurs when fixed oils or wax are projected in small quantities on a plate of metal heated considerably below redness; and from some experiments I am inclined to believe that this modification of combustion takes place in most cases where these substances are heated to ebullition in free contact with the air.

I have submitted the result of my observations on this subject in this imperfect state; as a long time might elapse before I could be enabled to extend them further, and in the expectation of seeing them prosecuted in abler hands.

I am, Sir, with respect, yours, &c.
CHARLES J. B. WILLIAMS.

Article XII.

Analyses of Books.

Transactions of the Royal Geological Society of Cornwall, Vol. II. 1822.

(*Concluded from* vol. v. p. 300.)

The third order of veins described by Mr. Carne is that of true veins, which consist of the following classes, enumerated in the order of their relative ages, as determined by the principle of intersection:

1. The oldest tin lodes; 2. The more recent tin lodes; 3. The oldest east and west copper lodes; 4. The contra copper lodes; 5. Cross courses; 6. The more recent copper lodes; 7. The cross flukans; 8. The slides.

"In describing the contemporaneous veins," he observes, "some were mentioned as occurring in the veinstones of other veins. There are also metallic veins which may be denominated veins within veins." These comprise black and grey silver ore, with native silver, in the copper lode of Huel Ann; wood-like and common tinstone in quartz and tinstone, the latter crossing the veinstones of the lodes; various ores of copper, in the quartz veinstones of Huel Carpenter, Huel Neptune, Huel Damsel, &c.; woodlike oxide of iron, in brown ironstone, at Botallack and Boscagel Downs; fibrous oxide of iron, in quartz, and veins of carbonate of iron at Huel Jubilee; and minute veins of native bismuth, in coarse red jasper, at Botallack.

Some remarks on the geological constitution of that part of Cornwall in which most of the veins described are found, and on the number and variety of the veins themselves, terminate the paper. After saying, "the claim of the granite which forms the chain extending from the Land's End to Brown-willy, has rarely, if ever, been seriously disputed;" and adverting to the supposition that St. Michael's Mount is transition granite, Mr. Carne continues:

"The claims of the clayslate, however, have of late been disputed, and it has been called transition slate, and greywacke slate, by geologists whose authority certainly carries considerable weight: but by what rules are we to distinguish primitive from transition slate? In its structure, the clayslate of Cornwall appears, in general, perfectly homogeneous. It contains no impressions of any kind. Some of the oldest metals have been found in it, viz. oxide of tin, wolfram, and mispickel; these have been discovered *in* the slate; and they have also been found, together with sulphuret of tin, native bismuth, and oxide of uranium, in the veins which intersect it. Some of the oldest

minerals have also been found in it; viz. axinite, garnet, topaz, &c. Are not these some of the strongest marks of a primitive rock? Some specimens of it were sent by a member of our Society to Werner, who recognized in them the clayslate of Saxony.

"That greywacke exists, and abounds in some parts of Cornwall, will not be denied; but I apprehend it is not to be found, except in small and scattered portions, in that part in which nearly all the tin and copper mines are wrought. It appears to commence near Grampound, and to extend westward about three miles from Truro: how far southward, I have not been able to ascertain. Its extent northward in mass is probably not from Truro; but it is found in bunches as far as Padstow, and Tintagel Castle. Near the former place, it is highly characterized; and at Tintagel impressions have been found in it. The lead veins of the Garres, and those of Pentire Glaze, and the vein of antimony at Huel Boys, are probably in greywacke; but I have never seen either copper or tin *in* greywacke, nor am I aware of any tin veins which intersect it."

VI. *Observations on the Submersion of Part of the Mount's Bay, and on the Inundation of Marine Sand on the north Coast of Cornwall.* By Henry Boase, Esq. Treasurer GSC.

In this paper, Mr. Boase first examines and refutes the tradition handed down by the historians of Cornwall, respecting the submersion of the country called Lionnesse: he then discusses that which relates to an irruption of the sea over a tract of low woodland, now forming the northern part of the Mount's Bay, showing that it is strongly corroborated by the geological indications of the district; and he concludes with an account of the overwhelming at an unknown period, by an immense inundation of sea-sand, of a considerable tract of cultivated land on the northern coast of the county.

VII. *On the Nomenclature of the Cornish Rocks.* By John Hawkins, Esq. FRS. &c. Hon. MGSC.

This communication commences with some remarks on the benefits conferred by Werner on the sciences of geology and mineralogy, which are succeeded by observations on the use of the term greywacke, in which the author suggests that the substance so denominated should be considered only as a subordinate formation to clayslate.

The characteristic appellations given by Werner to some of the specimens in a collection of the principal Cornish rocks, transmitted to him by Mr. Hawkins, form the next subjects of the paper, and it terminates with some reflections arising from the consideration of them.

VIII. *On the Temperature of Mines.* By John Forbes, MD. Hon. Mem. and late Sec. GSC.

IX. *Observations on the Hornblende Formation in the Parish of St. Clere.* By the Rev. John Rogers, MGSC.

The local details of which this paper consists are not susceptible of abridgment; but it contains the following analysis of the serpentine of Clickertor, by the late Mr. Gregor, which, we believe, has not hitherto been given to the public:

Silex.	42·2
Oxide of iron	19·6
Magnesia.	16·0
Alumina.	2·2
Lime.	0·8
Water	11·4
	92·2

X. *On the Phenomena of Intersected Lodes, and the legitimate Inferences which may be drawn from them;* and

XI. *On the Intersection of Lodes in the Direction of their Dip or Underlie.* By John Hawkins, Esq. FRS. &c. Hon. MGSC.

These interesting papers cannot usefully be epitomized.

XII. *On the Geology of the Land's End District.* By John Forbes, MD. Hon. Mem. and late Sec. GSC.

We have only room for a brief general sketch of the subject of this memoir,—the physical structure of that portion of Cornwall situated to the westward of a line drawn from the estuary of Hayle, on the north coast, to Cuddan Point on the south, which Dr. Forbes calls, for the sake of distinction, the Land's End district. "The geological structure of this district," he observes, "may be said to be very simple, inasmuch as it includes but a small number of rocks, and as the various relations of these to each other are very similar and readily discoverable. The main body of the district is *granite,*—a rock which indeed constitutes nine-tenths of the whole. On the edges of the granite, in different points of the coast, reposes a certain assemblage of rocks, which, from their intimate relations, obviously constitute *one formation.* These rocks I shall take the liberty of naming in this paper the *slate formation;* a term which answers exactly to the *killas country* of the miners and farmers in this part of the county. Generally speaking then, this district consists of two, and only two formations; the *granite formation,* and the *slate formation;* very dissimilar in appearance, and indeed very distinct in all their characters; although, as will hereafter be more particularly noticed, in all probability of contemporaneous origin."

"The *granite,* generally speaking, is *large-grained,* and very frequently possesses that particular arrangement of the crystals of felspar that entitles it to the epithet *porphyritic.* This character may indeed be said to be almost universal, and is exemplified in the pillars of almost every field gate.......Almost the only foreign ingredient (with the exception of the metallic ores in the neighbourhood of some veins and floors of tinstone), con-

tained in the granite of this district is *shorl.* This occurs in great plenty. In one spot, on the coast near Cape Cornwall, of several hundred yards in extent, this mineral forms so considerable a proportion of the rock, as to give it quite a new aspect, and has indeed procured it from geologists a distinct appellation, viz. *shorl-rock* . . . . . The locality now mentioned, and the celebrated Roach Rock, in the neighbourhood of Bodmin, are the only places in Cornwall where I have heard of this modification of granite being found in *mass.* In the form of veins, indeed, traversing both the granite and slate at their junction, it is very common."

The *slate formation,* "is much more complicated than the last, and affords much greater scope for geological research. It comprehends, as far as I have been able to ascertain, five distinct rocks. These are clayslate, hornblende rock, greenstone, compact felspar, and slaty felspar. . . . . . .By felspar rock, I mean a rock of small granular structure, consisting, apparently, principally, or almost wholly, of felspar. By slaty felspar, I mean a rock apparently of the same composition, or only with the addition of a very small portion of mica, with a distinct slaty fracture. These five rocks, constituting the assemblage to which I have given the name of the *slate formation,* occur in beds of various magnitude, alternating with each other; but with one very small exception, I have uniformly found the *slaty felspar rock* in immediate contact with the granite; and I think it not improbable that in proportion as we recede from this central rock, we shall find the slaty felspar become less frequent, and be finally superseded by some of the varieties of clayslate." . . . . "The rock which I have named compact felspar, which consists principally, I believe, of compact felspar with a little quartz, I have so named in deference to my excellent and learned friend Prof. Jameson: it may, however, be considered as a variety of greenstone. The only difference between it and common greenstone, is its containing a more minute portion of hornblende, and being, consequently, of a lighter colour than that rock generally is." . . . . . .The author has presented to the Society specimens of every rock which he has described.

XIII. *An Account of the Alluvial Depositions at Sandrycock.* By the late Philip Rashleigh, Esq.*

The alluvial beds described in this communication are very similar to those at the stream-work of Poth, of which an account was long since given by Mr. Rashleigh, at the end of his work entitled "Specimens of British Minerals."

XIV. *Observations on the Alluvial Strata at Poth, Sandrycock, and Pentuan.* By John Hawkins, Esq. FRS. &c. Hon. Mem. GSC.

These observations will not admit of profitable abridgment.

* Drawn up in 1797, and communicated by John Hawkins, Esq. Sept. 1819.

XV. *On the Mineral Productions, and the Geology of the Parish of St. Just.* By Joseph Carne, Esq. FRS. &c. MGSC.

The space to which our analysis must be confined obliges us to pass over Mr. Carne's enumeration of the minerals which have been discovered in this district, together with the greater part of his description of the peculiar geological facts observed in it: the only sections of the latter for which we have room are the following:

"*Floors, or Horizontal Beds.*—St. Just abounds in floors of tin, more than any other part of Cornwall.

"In that part of the tenement of Trewellard which is in a slate country, some tin floors have been wrought near the surface; the deepest is only seven fathoms below it: they were from one to two feet in thickness, and perhaps twenty feet in diameter;* they occurred at the junction of several tin lodes.

In Huel St. Just, a mass of tin ore, of a very singular nature, was discovered some years ago, which appears to belong to the floor formation. It first appeared at the depth of 17 fathoms under the sea, and has been followed downwards about 10 fathoms. It was seven or eight feet in diameter. At the top, it was on the south-western side of the tin lode; but it inclined in a very small degree, until it was almost wholly on the north-eastern side of the lode. The cavity in which it was found, had the appearance (after the tin was taken away) of a large underlying shaft, closed at the top. The most remarkable circumstance, however, relates to the state in which the tin ore was found; instead of being in a solid body, as is usual in floors, it appeared (as the miners termed it, from whom I received the account) like a heap of *attle*, or rubbish; just as if it had been thrown in that state into the cavity. The fragments were not rounded, but had all the appearance of the broken tinstone, which is generally seen on the surface of tin mines. The top of this mass of ore was about three feet below the granite top of the cavity, as if it had sunk by contraction or pressure. One of the miners told me that he found sufficient space between the granite covering and the ore, to sit upright on the latter. In its present deepest part, it is not so wide as it was at a higher level; but it is more compact. The tin ore, as raised from this cavity, contained, according to the miners' mode of calculation, from 700 to 1000 of tin to every 100 sacks. This floor, although of far greater thickness than any other which has yet been discovered, does not appear to be the result of the union of several lodes, for no such union takes place near it. Only one lode has been found connected with it, which, although perfectly distinct in the granite, both south-east and north-west of the floor, appears to lose its individual character, and to form one

* "It must not be supposed from this description that the floors are round: on the contrary, they are frequently very irregular, but their surface is about as large as would be comprised in a circle of 20 feet diameter."

body with it at the meeting. The floor, therefore, probably belongs to the same formation as the lode."

"Botallack is, however, the principal locality of the floors. Here they have been discovered, first, in slate. There is only one floor wholly in slate, which is 36 fathoms under the sea. It is about a foot thick, and occupies the space between a side lode and a neighbouring master lode, which is from 12 to 18 feet. No junction of lodes takes place at this spot. They occur, secondly, between the slate and the granite. Here, in a part of the mine called the Bunny, the principal floors have been found. The highest floor was so shallow as to be level with the surface, and tradition reports it to have been discovered by some of the tinstone having been kicked up by horses going over it: to this succeeded a floor of the country from one to three feet thick: then followed a second floor of tin, under which was found another floor of the country; and in this manner no less than seven floors of tin succeeded each other: the thickness of each was from six to twelve feet; some of them were full forty feet in diameter, but in general they were not so large. The country between the floors was generally slate, although they occurred just at the junction of the slate and the granite. At this spot there is a union of several lodes. It is singular that one of the marks by which the miners knew they were approaching a floor of tin, was their meeting with a floor of tourmaline, to use their own expression, 'the cockle rode on the tin.' Wherever they discovered the tourmaline, they were confident of finding a floor of tin under it. The tourmaline was accompanied by chalcedony, and I have seen veins of chalcedony running through it. Thirdly, in granite. In another part of Botallack, there are no less than 10 floors of tin, each as large as a space of about 30 feet square, succeeding each other in the same way as those which have been already described. The first was very little below the surface: the last is about 36 fathoms deep: they are from six to twelve feet thick. The agents of Botallack have assured me, that although these floors appear to be connected with one of the tin lodes, there is no junction of lodes in the space where they occur. In other parts of this mine, solitary floors have been found at different depths, on one of which, at 22 fathoms under the surface, the miners are now at work. It is about nine feet in diameter, and nearly round. They have seen its extent, and have found the country both above and below it (for it is quite horizontal) to consist of a very hard granite rock."

"These floors have generally been regarded as the result of the union of several lodes. This, however, is cutting a knot which is not easy to untie. As some floors have been discovered where no union of lodes has taken place, such a union does not appear absolutely necessary to their formation. In the case of a single floor of tin, not larger than those which have been

described, its formation may, perhaps, be accounted for on the same principles as the formation of all true veins; but where there is a succession of floors, if junctions of lodes could be satisfactorily shown at every point where they occur, it would give us little assistance in forming a theory of their formation. We have been accustomed to consider the contents of the tin lodes as of posterior formation to the rocks which contain them; but here is a succession of beds, all of them connected with tin lodes (for they are always found on one or both sides of tin lodes, to which, when they are not quite close, they are united by a small branch), and yet alternating with the rocks of the country, which are supposed to be older than the tin lodes. It is not surprising that the practical miner troubles himself little respecting the theory of the formation of the metalliferous bodies which he may discover; but it is indeed extraordinary (as Mr. Hawkins has observed in his paper on tin floors), that in a district where so large a quantity of tin has been found in floors, there is not more diligence and perseverance evinced in searching for those deposits."

"*Formation of Sandstone.*—In Pendeen Cove, which forms the northern boundary of this parish, the sand consists principally of comminuted shells, mixed with particles of slate, and of the constituent parts of granite. The cliff which bounds the cove is rather precipitous, and in one part consists of large fragments of granite imbedded in clay and earth. The interstices of this cliff are filled with sand (probably blown there from the beach by high winds); which is exposed to the percolation of water holding in solution the oxide of iron, whose cementing property is well known. The sand is thus gradually becoming stone, and in some parts of the cliff it has already acquired considerable hardness."*

"In the Cliff near Little Bounds Mine, the same operation is going forward, but the sand is more granitic than at Penden Cove."

XVI. *On the Knowledge and Commerce of Tin among ancient Nations.* By the Rev. Samuel Greatheed. (Communicated by John Dennis, Esq. MGSC.)

This paper is entirely archæological.

XVII. *On the Geology of St. Michael's Mount.* By John Forbes, MD. &c.

We can only quote some concluding passages of this commu nication:

"Much has been said respecting the relative age of different granitic rocks in different countries, and among others, respecting that of St. Michael's Mount, which has by some late writers

* "As this sand appears in some cases to extend further than the face or the interstices of the cliff, some have supposed a body of it to have covered the ancient surface, either by the means of high winds, or other causes, before the superincumbent mass of clay and granite fragments was placed there."

been stated to be of that class of rocks denominated *transition* by the Wernerian School. Of the existence any where of a class of rocks entitled to this name, I have great doubt; of the impropriety of considering the granite of St. Michael's Mount as of a different age and formation from that of the rest of Cornwall, I have no doubt whatever; and the appearances adduced by some writers as indicating posterior formation, are either imaginary or fallacious, or are common in other parts of the country, which are considered by these very geologists as primitive. Although the existence of strata of slate dipping *under* granite, and of beds or strata of granite *resting on*, and alternating with, slate, would not be a decisive proof, in my estimation, that one of those rocks was formed before the other; it is but justice to state that the accounts which describe such alternations as occurring at St. Michael's Mount, are totally erroneous; and I have no hesitation in saying, that there is no instance to be found in the whole of the Land's End district, where any thing like a bed of granite is found resting on slate.

I may here notice a circumstance that may tend to throw some light on the veined structure of St. Michael's Mount, that it shares this character with several other spots on these shores, where the main body of the granite is in contact with the slaty rocks. This is more especially remarkable at Polmear, in Zennor; and in the neighbourhood of the Logan Rock. Indeed, I consider these quartz veins, and the true shorl rock veins mentioned in a former paper, as mere varieties of the same substance."

XVIII. *On some Instances of the alternate Disposition of the primitive Strata which have been observed in Cornwall.* By John Hawkins, Esq. FRS. &c.

This article relates to an apparent alternation of granite and clayslate observed in several mines near the line of junction of those rocks, which has already been described in Mr. Thomas's Survey of the Mining District of the County; as well, we believe, as in other publications.

XIX. *On the Tin Ore of Botallack and Levant.* By Henry S. Boase, MD. Sec. GSC.

The processes of dressing and smelting the mixed tin and copper ores of Botallack and Levant, as described by Dr. Boase, present nothing remarkable, nor are his explanation and suggestions for the improvement of them possessed of greater interest, though calculated to be highly useful to those persons, practically engaged in such concerns, who are unacquainted with chemistry. The paper contains, however, the following interesting account of a specimen of tin pyrites, from a new locality:

"Here I would digress for a moment to notice a very interesting discovery, accidentally made, of tin pyrites, which has been no where found, I believe, except at Huel Rock, in St. Agnes; Slenna-gwyn, in St. Stephens; and Huel Scorier,

in Gwennap. I had desired a workman employed at the stamping-mill and burning-house at Botherris, to send me three specimens of tin ore containing copper, one of which I found to be an aggregation of yellow copper ore and tinstone; another of grey copper ore and tinstone; but the third, to my great surprise, had a compact uniform structure, perfectly homogeneous in appearance, resembling tin pyrites in all its external characters; and on comparing it with the specimens in our cabinet, it agreed in every respect, except that its colour was a little lighter, with rather more of metallic lustre. To determine its nature with greater certainty, this substance was submitted to the following experiments: When exposed to a red heat in a covered crucible, it lost weight, and sulphur was sublimed: calcined with free admission of air, sulphurous acid gas was evolved; it increased in bulk; changed to a dark-brown colour, and lost 15 per cent. in weight. In nitromuriatic acid it readily dissolved without the application of heat, and during solution, nitric oxide gas was disengaged. Intending subsequently a more accurate analysis, a rough one was performed after the method proposed by Klaproth. The result was, in 100 parts:

Copper	31
Tin	28
Iron	6
Sulphur	25
Silica, with a little alumina	7
Loss	3
	100

The loss was probably occasioned by some of the sulphur (during the solution of the mineral in the acid) escaping in the form of sulphuric acid gas. This analysis proves beyond doubt that the mineral was tin pyrites."

Dr. Boase was unable to procure even another specimen of this mineral from Botallack; but it appears that the one just described came from that part of the mine which is called Huel Hazard.

XX. *On the Temperature of the Cornish Mines.* By M. P. Moyle, Esq. MGSC.

XXI. *On the Serpentine District of Cornwall.* By the Rev. John Rogers, MGSC.

This paper, like the former one by the same author, consists of local details unsusceptible of abbreviation: they are chiefly confined to some circumstances of the interesting district in question, which, Mr. Rogers states, have escaped the notice of Mr. Majendie and of Prof. Sedgwick, in their respective surveys of it.

A series of tables of the quantities of tin and copper raised in Cornwall in different years, those of the former metal com-

mencing in the year 1750, and ending in 1821, and those of the latter beginning in 1771, and terminating in 1822, with several others of the quantities of copper produced by the various mining districts of the kingdom from 1818 to 1822; a list of donations to the Society; and another of the minerals wanted to complete its cabinet, close the volume. B.

Narrative of a Journey to the Shores of the Polar Sea, in the Years 1819, 1820, 1821, *and* 1822. By John Franklin, Capt. RN. FRS. and Commander of the Expedition. *With an Appendix on various Subjects relating to Science and Natural History. Illustrated by numerous Plates and Maps. Published by Authority of the Right Honourable the Earl Bathurst.*

(*Concluded from* vol. v. p. 387.)

We intended, in the present article, to have given a minute analysis of the Appendix to Capt. Franklin's Narrative; omitting any notice of that narrative itself, on account of the numerous channels of general information through which the public either are or will be made acquainted with its contents. Such, however, is the variety and importance of the scientific information comprised in the Appendix, occupying *two hundred and seventy closely printed quarto pages*, that it would be impossible, within the space allotted to this department of the *Annals*, to give even the semblance of a detailed account of it. The utmost we can do, therefore, is to present our readers with an enlarged table of the contents of this Appendix; and as we inserted two papers from it on the Aurora Borealis, in the commencement of the article, we will subjoin a few observations selected from several others; in order that the reader may possess some satisfactory knowledge, of at least one of the subjects principally treated of, by the indefatigable traveller and his no less indefatigable coadjutors. At some future opportunity, perhaps, we may transfer to our pages some further portions of their labours:

The following are the contents of the Appendix in question:

No. 1.—Geognostical Observations; by John Richardson, MD. and Surgeon to the Expedition. 41 pages.

No. 2.—Aurora Borealis. 9 pages; including Capt. Franklin's General Remarks, and Lieut. Hood's Observations, given in the *Annals* for May;—with An Account of the Aurora Borealis seen at Cumberland House between Oct. 23, 1819, and June 13, 1820, by the latter officer;—and Observations on the Magnetic Needle at Cumberland House, from the beginning of Feb. to the end of May, 1820, by the same.

No. 3.—Observations on the Aurora at Fort Entreprise; and Notices of the Appearances of the Aurora, at the same place; both by Capt. Franklin:—Table of Observations on the Devia-

tions of the Magnetic Needle, made at Fort Entreprise, from Jan. 12 to April 9, 1821:—On the Aurora Borealis at Fort Entreprise; Appearances of the Aurora at the same place; and a Table of the Diurnal Variation of the Needle there; all by Lieut. Hood:—Remarks on the Aurora Borealis, by Dr. Richardson. In all 79 pages.

No. 4.—Remarks and Tables connected with Astronomical Observations; 17 pages: including, Three Tables of the Diurnal Variation;—General Remarks on the Variation of Kater's Compasses, observed during the Journey in North America, and along the Arctic Sea;—Results of the Observations for Latitude, Longitude, and Variation;—Table of Observations on the Dip of the Magnetic Needle, between York Factory and Point Turnagain;—Table of Observations on the Magnetic Force;—Tables of Temperatures;—General Tabular View of the Winds and Weather for One Year, 1820, 1821;—Various Observations on the Passage to Hudson's Bay.

No. 5.—Zoological Appendix; by J. Sabine, Esq. 56 pages:—Quadrupeds;—Birds.

No. 6.—Notices of the Fishes; by Dr. Richardson. 24 pages.

No. 7.—Botanical Appendix; by Dr. Richardson. 40 pages; describing 663 species of plants:—Addenda, by Robert Brown, FRS.

We proceed to select some observations on the Aurora Borealis: the following are by Capt. Franklin, made at Fort Entreprise, in lat. 64° 28′ 24″ N.; long. 113° 6′ 0″ W.

"The arches of the Aurora most commonly traverse the sky, nearly at right angles to the magnetic meridian, but the deviations from this direction were not rare; and I am inclined to consider, that these different positions of the Aurora have considerable influence upon the direction of the needle. When an arch was nearly at right angles to the magnetic meridian, the motion of the needle was towards the west; this westward motion was still greater when one extremity of an arch bore 301°, (or about 59° to the west of the magnetic north), that is, when the extremity of the arch approached from the west towards the magnetic north. A westerly motion also took place when the extremity of an arch was in the true north, or about 36° to the west of the magnetic north, but not in so great a degree as when its bearing was about 301°. A contrary effect was produced when the same end of an arch originated to the southward of the magnetic west, viz. when it bore from about 245° to 234°; and, of course, when its opposite extremity approached nearer to the magnetic north. In these cases, I say, the motion of the needle was towards the east."

"In one instance only, a complete arch was formed in the

magnetic meridian; in another, the beam shot up from the magnetic north to the zenith; and in both these cases, the needle moved towards the west."

"The needle was most disturbed on February 13 [1821], p.m. at a time when the Aurora was distinctly seen passing between a stratum of clouds and the earth, or at least illuminating the face of the clouds, opposed to the observer. This and several other appearances, recorded in the accompanying notes, induced me to infer that the distance of the Aurora from the earth varied on different nights, and produced a proportionate effect on the needle. When the light shone through a dense hazy atmosphere, when there was a halo round the moon, or when a small snow was falling, the disturbance was generally considerable; and on certain hazy cloudy nights, the needle frequently deviated in a considerable degree, although the Aurora was not visible at the time. Our observations do not enable us to decide whether this ought to be attributed to an Aurora concealed by a cloud or haze, or entirely to the state of the atmosphere. Similar deviations have been observed in the day-time, both in a clear and cloudy state of the sky, but more frequently in the latter case. Upon one occasion, the Aurora was seen immediately after sunset, while bright day-light was remaining."

"A circumstance to which I attach some importance must not be omitted. Clouds have been sometimes observed during the day to assume the forms of the Aurora, and I am inclined to connect with the appearance of these clouds the deviations of the needle, which was occasionally remarked at such times."

"An Aurora sometimes approached the zenith, without producing any change in the position of the needle, as was more generally the case, while at other times a considerable alteration took place, although the beams or arches did not come near the zenith. The Aurora was frequently seen without producing any perceptible effect on the needle. At such times its appearance was that of an arch or an horizontal stream of dense yellowish light, with little or no internal motion."

"The disturbance in the needle was not always proportionate to the agitation of the Aurora, but it was always greater when the quick motion and vivid light were observed to take place in a hazy atmosphere."

"In a few instances, the motion of the needle was observed to commence at the instant a beam darted upwards from the horizon. And its former position was more quickly or slowly regained according to circumstances. If an arch was formed immediately afterwards, having its extremities placed on opposite sides of the magnetic north and south to the former one, the return of the needle was more speedy, and it generally went beyond the point from whence it first started."

"When the disturbance of the needle was considerable, it

seldom regained its usual position before three or four p.m. on the following day."

"On February 13, at 11^h 50^m p.m., the needle had a quick vibratory motion between 343° 50′ and 344° 40′. This is the only occasion on which a vibratory motion was observed."

"The disturbances produced by the Aurora were so great, that no accurate deductions could be made respecting the diurnal variation."

"I have not heard the noise ascribed to the Aurora, but the uniform testimony of the natives and of the residents in this country, induces me to believe that it is occasionally audible. The circumstance, however, must be of rare occurrence, as is evidenced by our having witnessed the Aurora upwards of two hundred times without being able to attest the fact. I was almost inclined, last year, to suppose that unusual agitations of the Aurora were followed by storms of wind; but the more extended opportunities I enjoyed of observing it in 1821, at Fort Entreprise, have convinced me that no such inference ought to have been drawn."

"The Pith Ball Electrometer, which was placed in an elevated situation in the air, never indicated an atmosphere charged with electricity." P. 551—553.

The succeeding remarks and experiments on this curious subject were made at Fort Entreprise, by the ill-fated Lieut. Hood, and are extracted from his Journal.

"On the 27th of April, 1821, at 10^h 30^m p.m., a single column of Aurora rose in the north, and traversed the zenith towards the south; another column appearing, NE by E and taking a parallel direction. The frost was slightly agitated, and the beams momentarily visible. It passed to the western horizon in ten minutes, and was followed by the other, which became brighter as it approached the zenith. I am now convinced they were borne away by the wind, because the columns preserved exactly their distance from each other during their evolution; and some detached wreaths, projected from them, retained the same relative situations of all their parts; which never happens when the Aurora is carried through the air by its own direct motion. The wind was E by N, a strong gale, and the temperature of the air 9°."

"It must be admitted that the influence of the wind upon the Aurora was never suspected until the 27th of April. However, there are several particulars connected with the subject, which may have prevented such an influence from manifesting itself on former occasions. 1st. When the coruscations were rapid and brilliant, they forced themselves against the wind, or in the contrary direction, without any perceptible difference of speed; from which circumstance, I was led to suppose that they were not in any degree affected by the wind, and did not afterwards pay sufficient attention to discover my error.

2d. The prevailing winds were from the eastward and westward; and the arches usually extending from NW to SE; the influence of the wind might have been mistaken for their lateral motion. 3d. The northerly winds, acting from the same quarter as the direct motion, were confounded with it. Lastly, the southerly winds, which were not common, always filled the atmosphere with clouds, so that the Aurora was not visible. Perhaps, after all, the Aurora of the 27th of April was nearer to the earth than any other which we saw."

"On the 11th of March, at 10^h p.m., a body of Aurora rose NNW, and after a mass of it had passed to E by S the remainder broke away, in portions consisting each of several beams, which crossed about 40° of the sky with great rapidity. We repeatedly heard a hissing noise, like that of a musket-bullet passing through the air, and which seemed to proceed from the Aurora; but Mr. Wentzel assures us, that this noise was occasioned by severe cold, succeeding mild weather, and acting upon the surface of the snow, previously melted in the sun's rays. The temperature of the air was then 35°, and on the two preceding days, it had been above zero. The next morning it was − 42°, and we frequently heard a similar noise. Mr. Hearne's description of the noise of the Aurora agrees exactly with Mr. Wentzel's, and with that of every other person who has heard it. It would be an absurd degree of scepticism to doubt the fact any longer; for our observations have rather increased than diminished the probability of it." P. 584, 585.

"The common cork-ball electrometer not having on any occasion given signs of a charge, I tried the following experiment, in order to attain further evidence on the subject. A brass needle was attached to a compass card, and balanced on a copper pivot in a wooden box. It was about four inches in radius, and a copper arch of 60° to that radius, was fixed at one end of the box, which was closed by a wooden slide, and paper pasted over every crevice to exclude the air. To give it the same advantages for conducting electricity as the compass boxes (which are made of brass), I introduced an iron wire, eight inches in length, perpendicularly through the lid, in such a manner, that its lower extremity was in a horizontal plane with the needle; and a pane of glass at that end of the lid, enabled me to see into the interior of the box. Having previously ascertained that it contained no magnetism, the instrument was placed, on the 2d of May [1821], on a covered shelf, at the outside of the house, in a position nearly east and west; the brass needle being 25′ from the conductor, and a small glass bubble adjusted on the box, in order to prevent its otherwise unperceived movement. At 12^h p. m. I examined the needle, and found its position unaltered. No Aurora was then visible, but one was afterwards seen by Mr. Franklin;

and at 8^h a. m., May 3d, the needle and conductor were in contact. I moved the needle 40′ from the conductor, and it was similarly affected at some period on the nights of May 3d, 5th, 6th, 9th, 10th, and 11th. The thermometer, during this period, ranged in the day between $+26°$ and $+56°$; and in the night, between $+10°$ and $+33°$. I did not see the Aurora, except on the nights specified above; and did not perceive any alteration in the needle till the succeeding mornings."

"The night of the 12th furnished a more satisfactory proof of the agency of the Aurora. At 10^h p. m. the needle was not affected, and no Aurora was visible. At 0^h 30′ a. m. May the 13th, several arches appeared across the sky from NW to SE, and the needle was attracted to the conductor from the distance of 1°. The temperature of the air was $+12°$. I now determined to convert the instrument into a kind of electrometer, by insulating the needle and conductor. The pivot which supported the former was fixed upon sealing wax, and the point of the latter, which passed through the lid, was covered with the same substance."

"Paper was pasted on the box as before, and it was re-placed at 2^h p. m. on the 14th, the temperature of the air being 54°. A heavy gale of wind from NNW, with snow, immediately followed, and the temperature of the air, at midnight, was reduced to 19°. At 9^h a. m. May 15th, the needle was removed 30° from the conductor, and both were still charged, so that I could not bring them together till the conductor was accidentally touched. I believe this change to have been received from an Aurora; because the same weather, preceding and following it, did not affect the needle in the day, when the increased warmth of the air was more favourable to the production of electricity in other quarters, and also to its passage. On the 24th of May, between 10^h and 12^h p. m. the needle was attracted to the conductor, and repelled 25°.* The next morning, Mr. Franklin found the needle of the transit instrument (which was then in the meridian) affected 20′. The brightness of the twilight prevented us from seeing the Aurora, and I therefore discontinued my observations."

"That electricity was the cause of the motions which I have described does not admit of a doubt. But whether the electricity was received from, or summoned into action by, the Aurora, my readers will determine for themselves, being in possession of the facts upon which I have myself founded my opinion." P. 586, 587.

Dr. Richardson is of opinion, that, independently of all theory, his notes "will at least serve to prove that the Aurora is occasionally seated in a region of the air, below a species of cloud which is known to possess no great altitude. I allude to

* "The thermometer was then 20°, and at 3^h p. m. it had been 58°."

that modification of cirro-stratus, which, descending low in the atmosphere, produces a hazy continuity of cloud over-head, or a fog bank in the horizon. Indeed, I am inclined to infer, that the Aurora Borealis is constantly accompanied by, or immediately precedes, the formation of one or other of the various forms of cirro-stratus. On the 13th of November, and 18th of December [1820], its connexion with a cloud intermediate between cirrus and cirro-stratus is mentioned; but the most vivid coruscations of the Aurora were observed when there were only a few attenuated shoots of cirro-stratus floating in the air, or when that cloud was so rare that its existence was only known by the production of a halo round the moon. The bright moonlight of December was peculiarly favourable for observations of this kind. Had the nights been dark, many of the attenuated streaks of cloud hereafter mentioned would have een totally invisible." P. 597.

"I think I have on some occasions discerned," Dr. Richardson continues, "a polarity in the masses of cloud belonging to a certain kind of cirro-stratus, which approaches to cirrus, by which their long diameters, having all the same direction, were made to cross the magnetic meridian nearly at right angles. The apparent convergence of such masses of cloud towards opposite points of the horizon, which has been frequently noticed by meteorologists, is of course an optical deception, produced when they lie in a plane parallel to that on which the observer stands. These circumstances are here noticed, because if it should be hereafter proved that the Aurora depends upon the existence of certain clouds, its apparent polarity may, perhaps, with more propriety, be ascribed to the clouds themselves which emit the light; or, in other words, the clouds may assume their peculiar arrangement through the operation of one cause (magnetism for instance), while the emission of light may be produced by another, a change in their internal constitution perhaps, connected with a motion of the electrical fluid. .... Generally speaking, the Aurora appeared in small detached masses for some time before it assumed that convergency towards the opposite parts of the horizon, which produced the arched form. An observation that I would connect with the previous remarks, by saying that it was necessary for the electric fluid (or the Aurora, if they are the same) to operate for some time before the polarity of the thin clouds, in which it has its seat, is produced."

"An electrometer, constructed upon Saussure's plan, placed in an elevated situation out of doors, exhibited no signs of a charge from the atmosphere at any time during the winter. The electricity of our bodies, however, at times was so great, that the pith balls instantly separated to their full extent upon approaching the hand to the instrument; and our skins were in the middle of winter so dry, that rubbing the hands together

considerably increased their electricity, and at the same time produced a smell similar to that which is often perceived when the cushion of an electrifying machine rubs against the cylinder." P. 598, 599.

The Aurora did not often appear immediately after sun-set. It seemed that the absence of that luminary, for some hours, was in general required for the production of a state of atmospere, favourable to the generation of the Aurora. On one occasion only (March 8th, 1821), did I observe it distinctly, previous to the disappearance of day-light." P. 599.

"I have never heard any sound that could be unequivocally considered as originating in the Aurora; but the uniform testimony of the natives, both Crees, Copper Indians, and Esquimaux, and of all the older residents in the country, induces me to believe that its motions are sometimes audible. These instances are, however, rare; as will appear, when I state that I have now had an opportunity of observing that meteor for upwards of two hundred different nights." Ibid. B.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

May 29.—At this meeting, the reading of Mr. W. S. Harris's Account of a Magnetic Balance, and of some recent Experiments on Magnetic Attraction, was resumed and concluded.

The construction of the magnetic balance is analogous to that of the electrical balance, described by Mr. Harris in his Observations on the Effects of Lightning on Floating Bodies, lately published: the experiments made with it were on the laws which govern the force of attraction in magnetized bodies, under different circumstances of distance, &c.

At this meeting, also, the reading of the following paper was commenced: A Case of Pneumato Thorax, with experiments on the absorption of different kinds of air introduced into the pleura; by John Davy, MD. FRS.

June 5.—The reading of Dr. Davy's paper was resumed and concluded.

The case described by Dr. Davy was one of Phthisis Pulmonalis, which proved rapidly fatal, owing to the supervention of Pneumato Thorax. A few hours after death, the chest was perforated under water, and nearly 226 cubic inches of air were collected from the right pleura, into which it had passed by means of an ulcerated opening communicating indirectly through a vomica with the bronchia. This air was found to consist of

azote and carbonic acid; about 94 of the former, and 6 of the latter.

For the purpose of elucidation, Dr. Davy described the results of a number of experiments which he had made on dogs, proving that different gases introduced into the pleura are absorbed with different degrees of rapidity. Nitrous gas, nitrous oxide, oxygen, and hydrogen, soon disappearing, carbonic acid gas more slowly, and azote slowest of all.

Some of the experiments gave rise to the idea that azote was effused into the pleura by the secernent arteries. This subject is discussed by Dr. Davy in connexion with the consideration of the air occasionally found by anatomists in different parts of the body. This air, for reasons which he assigns, he thinks is azote. He does not believe that it is carbonic acid gas, because he has been able to detect the slightest traces of this acid in blood either by means of a high temperature, or the vacuum of an air-pump, and because blood contains alkali not saturated with this acid, and is able, in consequence, to combine with an additional portion of it.

At this meeting a paper was also read, on Fossil Shells; by L. W. Dillwyn, Esq. FRS. in a letter to the President.

This paper principally related to the geological distribution of turbinated univalves.

At this meeting likewise, the reading of the following paper was begun: Observations and Experiments on the Daily Variation of the Horizontal and Dipping Needles, under a reduced directive Force; by Peter Barlow, Esq. of the Royal Military Academy, FRS. *elect.* (Communicated by Davies Gilbert, Esq. Treas. RS.)

June 12.—The reading of Prof. Barlow's paper was resumed and concluded.

A century has now elapsed, Prof. Barlow observed, in the commencement of this paper, since Mr. Graham discovered the diurnal variation of the needle, and, during this period, a number of observations upon it have been made by others, but none of them have led to any decided results respecting the general nature and laws of the phenomenon. Two years ago, the Royal Academy of Copenhagen proposed a prize question on the subject, which has not yet been claimed.

It occurred to the author, that if he could reduce the action of the terrestrial magnetism upon the needle, as mineralogists and others had long been in the habit of doing, for the purpose of detecting very small quantities of magnetism, the diurnal variation would then become more considerable. By pursuing this idea, the most convenient method of executing which he found to be the presenting of one pole of a magnet to the similar pole of the needle, and the opposite pole of another magnet to the opposite pole of the needle, he was enabled successively to increase the diurnal variation from a few minutes to 3° 40′, then

to 7° 0′, and so on to almost any quantity at pleasure. By approaching his opposing magnets nearer to each other and to the needle, the latter might, moreover, be deflected to any point, and by this means the daily variation observed with the needle in all possible positions. In this way the author found the daily variation, with the north end to the south, to the east, west, &c. &c. and it appeared that the daily change was always greatest with the needle east or west, and least (indeed imperceptible) when the needle pointed any where near NNW and SSE. From the NNW to south, the principal daily motion was shown by the north end approaching the north, and between the SSE and N, the north end still approached the north and NNW, and, therefore, the motion in the two cases was made in a reverse order. Similar experiments were made on the dipping needle, but the results were not so well marked.

From a comparison of these experiments, Mr. Barlow is inclined to attribute the cause of the daily variation to a change of magnetic intensity in the earth produced by the action of the solar rays, and depending for its amount upon the declination of that body; and consequently on its situation with reference to the plane of no attraction as described in his Essay on Magnetic Attractions, where he has stated his reasons for assuming that the cause, whatever it may be, that gives direction to the needle, is resident on its surface only.

A singular anomaly in the diurnal variation under a reduced directive force, was described in the latter part of the paper: a compass-needle which varied, in Mr. Barlow's house (with the north end of the needle to the east or west), to north, varied, in the garden, from east or west to south. Only three suppositions could be made as to the cause of this anomaly; first, that it might arise from the circumstance that the needle was not exactly in the same relative position with respect to the magnets, &c. in the house as in the garden; secondly, the window of the room where the compass was placed being on the north side, the light might thence affect the needle; or, lastly, was it possible that a stove in the room could experience a diurnal increase and diminution of magnetic power? In order to examine the first of these suppositions, Mr. Barlow carefully measured and determined the position of the needle, &c. in the one situation, and gave them precisely the same in the other, but the discrepancy still remained: he then completely darkened the room for two days, and merely examined the compass with a wax taper, but the former effect was only diminished by this means; the author is of opinion, however, from the result of this experiment, that the light, and not the heat of the sun, will be found the exciting cause of the diurnal variation: in order to examine the third supposition, Mr. Barlow placed a howitzer shell in the garden in the same position with respect to the needle as the stove was in the

house; this changed the period of the maximum effect from eleven o'clock in the morning till four in the afternoon; but the discrepancy continued, and consequently remains unaccounted for. The same difference of variation in the two situations was also found by Mr. Christie, whose house is at some distance from Mr. Barlow's, and who, at the suggestion and request of Mr. B. carried on a similar but totally distinct series of observations, and was led to the same results without being aware that they had occurred to Mr. Barlow.

The following paper was also read: On Bitumen in Stones; by the Right Hon. George Knox, FRS.

The results of Mr. Knox's experiments on the pitchstones of Newry and Meissen, already before the Society (Phil. Trans. 1822, p. 313; *Annals*, N. S. iv. p. 460), had induced him to submit a great number of other minerals to similar trials. Among these, the following yielded various proportions of bitumen and water: *Pitchstone*, from the Isle of Arran lost 4·705 per cent. by distillation, about 3 of which were bitumen, and the residuum, as in many other cases, was pumice; *pearlstone* from Tokay, in Hungary; *obsidian* yielded much bitumen, as did the *basaltic greenstone*, which forms a vein in the granite of Newry, parallel to that of pitchstone; *basalt* from Disko Island, and from the Giant's Causeway; *wacke* from Disko Island yielded 11 per cent. of bitumen; *iron clay* from Howth; *bole* from Disko Island; *menilite* from Menil-montant; *adhesive slate* from the same place; *common serpentine* from Zoëblitz in Saxony; *mica slate* yielded a small quantity of bituminous water; *clayslate* from Bangor; *fetid quartz* from Nantes gave 2 per cent. of bituminous water; *felspar* from Aberdeen, a little.

The following substances sustained no loss of weight by distillation: *pumice* from Lipari fused; *rock crystal* underwent no alteration; a colourless crystal of *adularia*.

Mr. Knox states, as the general result of his researches, that nearly all the minerals belonging to Werner's floetz-trap formation, contain bitumen; and that it likewise exists, but in smaller quantity, and more difficultly separable, in some of the substances which constitute the older rocks.

The paper concluded with some remarks on the new precautions in the analysis of stones, which the author's experiments just noticed seem to indicate the necessity of; since it would appear that the loss of weight by ignition, generally estimated as water, may, in reality, be partly owing to the expulsion of bitumen.

June 19.—As this was the last meeting of the Society for the present session, little more than the titles of the following papers could be read:

On Astronomical Refraction; by J. Ivory, Esq. FRS.

Tables of certain Deviations which appear to have taken

place in the North Polar Distances of some of the principal fixed Stars; by J. Pond, Esq. FRS. Astronomer Royal.

On a Case of Pneumato Thorax, in which the operation of tapping the chest was performed, with additional observations on air found within the body, and on the absorption of air by mucous membranes; by J. Davy, MD. FRS.

On the Length of the Invariable Pendulum in New South Wales; by Sir Thomas Brisbane, KCB. FRS.: communicated by Capt. Kater, FRS.: in a letter to the President.

Astronomical Observations made at Paramatta; by Mr. Rumker: communicated by Sir T. Brisbane, in a letter to the President.

Of the Motions of the Eye, in Illustration of the Uses of the Muscles of the Orbit; by Charles Bell, Esq. Part II.: communicated by the President.

On Algebraic Transformation, as deducible from first Principles, and connected with continuous Approximation, and the Theory of Finite and Fluxional Differences, &c.; by W. G. Horner, Esq.: communicated by Davies Gilbert, Esq. Treas. RS.

On the Apparent Magnetism of Metallic Titanium; by W. H. Wollaston, MD. VPRS.

In Dr. Wollaston's former paper on the minute cubes of metallic titanium contained in the slag of the iron works of Merthyr Tydvil (see Philosophical Transactions for 1823, Part I.; or *Annals of Philosophy* for January last, p. 68), he had stated that they were slightly magnetic; for although they were not taken up by a magnet, yet if one of them was suspended by a thread, the action of the magnet would draw the thread upwards about 20°, indicating an attractive force equal to about one-third of the weight of the crystal. By a comparative experiment, he found that 1-250th part of iron would impart equivalent magnetic power to metallic substances, and by repeated solution and evaporation, succeeded in removing so much of the titanium as to discover, in the edges of the precipitate by tincture of galls, the black colour of gallate of iron. It remains a question, therefore, whether these cubes of titanium are properly magnetic themselves, or whether they derive their magnetism from the minute portion of iron which they contain.

An Account of the Effect of Mercurial Vapours on the Crew of H. M. S. Triumph, in the year 1810; by William Burnett, MD.: communicated by Matthew Baillie, MD. FRS.

Contributions towards a Natural and Œconomical History of the Cocoa-nut Tree; by H. Marshall, Esq.: communicated by Sir James Macgregor, Bart. FRS.

On the Diurnal Variation of the Horizontal Needle, when under the Influence of Magnets; by S. H. Christie, Esq. MA. Mem. Cam. Phil. Soc. and of the Royal Military Academy: communicated by the President.

The President announced some alterations in the statutes of

the Society that have been made by the Council in their recent revision of them; by one of which, the meetings of the Society will commence, in future, for each Session, on the first of the two Thursdays preceding the Anniversary, and terminate on the third Thursday in June.

The Society then adjourned accordingly, *to Thursday* the 20th of November next.

A paper on the Compressibility of Water, Air, and other Fluids; and on the Crystallization of Liquids, and the Liquefaction of Aëriform Fluids, by simple pressure, was prepared by Mr. Perkins, for the purpose of submitting it to the Royal Society; but it was accidentally misplaced, previously to the last meeting, and therefore could not be announced to the Society with the other papers. It contained, we are informed, a minute description, accompanied with figures, of his compressing apparatus; a diagram, showing the ratio of the compressibility of water, beginning at the pressure of 10 atmospheres, and proceeding regularly to that of 2000; and some experiments on the compression of atmospheric air, which appears by them to follow a law varying from that generally assigned to it by philosophers. Mr. Perkins intended to announce, also, in this paper, that he had effected the liquefaction of atmospheric air, and other gaseous substances, by a pressure equal to that of about 1100 atmospheres; and that he had succeeded in crystallizing several liquids, by simple pressure.

ASTRONOMICAL SOCIETY.

May 9.—At this meeting, a paper on the Mercurial Compensation Pendulum, by Francis Baily, Esq. FRS. was read, but owing to its length, it could not be completed.

June 13.—The reading of Mr. Baily's paper on the Mercurial Compensation Pendulum was resumed and concluded. It contains an account of many experiments made to determine the rates of expansion of the various substances used in the construction of such pendulums, the results of which are given in a tabular form. The expansions of mercury, as given by different authors, are collected, and it is shown that none of them can be safely applied to the purposes of the pendulum without certain modifications which are pointed out in this paper. The principles of the Compensation Pendulum are then investigated, and a formula deduced for determining the height of the quicksilver in the cylinder, the result of which is different from those given by preceding writers on this subject. Mr. Baily then points out some improvements in the usual mode of constructing and regulating pendulums, which appear very simple and efficacious; and concludes his paper by the description of a compensation pendu-

lum, of great cheapness, being formed of wood and lead alone, but which, he states, may be made available for many useful purposes.

The Society then adjourned to Friday the 14th of November next.

We have heard with pleasure that the Council has awarded several gold and silver medals to be presented by the Society at one of its future meetings to some of the continental astronomers, for their discoveries; and a gold medal to Mr. Babbage, as a token of their high estimation of his invaluable invention of applying machinery to the computation of astronomical and mathematical tables. As soon as we receive correct information, we shall lay the particulars of these honorary tokens before our readers.

MEDICO-BOTANICAL SOCIETY OF LONDON.

April 25.—A paper, on the Essential Oil of Bitter Almonds, was read, by Mr. Frost, and Experiments were made (before the Society) on Animals with the Oil.

At this meeting a paper was also read, on Atropa Belladonna.

May 9.—Mr. Frost delivered a lecture on Stalagmitis Cambogioides, and Acorus Calamus.

A paper was also read from P. J. Brown, Esq. Corresponding Member of the Society, on several Medicinal Plants used by Swiss Practitioners.

Article XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Letter from Mr. Faraday, respecting the Historical Sketch of Electromagnetism published in the Annals.*

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

You did me the favour to insert in the second and third volume of the *Annals of Philosophy*, a paper which I had written, entitled, " A Historical Sketch of Electromagnetism." To that paper, the initial of my Christian name only was affixed. Wishing now, for reasons which will shortly be made public, to acknowledge myself as the author of it, I will thank you to insert this letter in the *Annals* as an assent on your part to the correctness of the statement which it contains.

I remain, dear Sir, yours, very truly,

M. Faraday.

II. *Diurnal Variation of the Magnetic Needle.*

We understand that Mr. Christie has continued to pursue his inquiries on this subject, as noticed in our report of Mr. Barlow's paper, and that he has been led to conclude from them, that it is the calorific and not the colorific rays that produce the change in question. He has found that a change of temperature in his opposing magnets, to the amount of one degree only, will produce a change of nearly a degree in the direction of the needle. He showed by the most satisfactory experiments, before Professors Oersted and Barlow, that the mere change of heat produced by applying his hand to the magnet, when the needle was thus nicely adjusted, caused a deviation to the amount of between two and three degrees.

Mr. Christie has communicated the first part of his experiments to the Royal Society, as announced in our report of the final proceedings of that body for the present Session.

III. *Frauds and Imperfections in Paper-making.*

In order to increase the weight of printing papers, some manufacturers are in the habit of mixing sulphate of lime or gypsum with the rags to a great extent. I have been informed by authority, upon which I place great reliance, that some paper contains more than one-fourth of its weight of gypsum; and I lately examined a sample which had the appearance of a good paper that contained about 12 per cent.

The mode of detecting this fraud is extremely simple: Burn 100 grains, or any given weight of the paper in a platina, or earthern crucible, and continue the heat until the residuum becomes white, which it will readily do if the paper is mixed with gypsum. It is certainly true that all paper contains a small quantity of incombustible matter derived from accidental impurities, but it does not amount to more than about one per cent.; the weight then will indicate the extent of the fraud.

With respect to the imperfection of paper, I allude to the slovenly mode in which the bleaching by means of chlorine or oxymuriatic acid is effected. This, after its operation, is frequently left in such quantity in the paper that it may be readily detected by the smell. Sometime since, a button-maker in Birmingham, who had manufactured the buttons in the usual way, was surprised to find that after being a short time kept, they were so tarnished as to be unsaleable; on searching for the cause, he found that it was derived from the action of the chlorine which had been left in the paper to such an extent as to act upon the metallic buttons.—*Edit.*

IV. *Boiling Spring of Milo.*

The 14th volume of the *Annals*, p. 27, contains an analysis of the water of the boiling springs of Milo; but this island is there incorrectly called Milto. For this correction I am indebted to the Rev. Mr. Holme, of Cambridge, by whom the water was supplied for analysis. —*Edit.*

V. *Crystals formed in Solution of Cyanogen.*

M. Vauquelin observed that a strong solution of cyanogen which he kept in his laboratory during the winter, became in about four months of

a light amber colour, and deposited crystals of an orange-yellow colour, the quantity of which increased for some time. The solution in which they were formed was examined; it had a strong smell of hydrocyanic acid, and was alkaline; it gave a bluish-green precipitate with sulphate of iron, which a drop of sulphuric acid immediately rendered blue. From these effects, M. Vauquelin concludes, that the solution of cyanogen was converted into hydrocyanate of ammonia.

The crystals obtained were dendritical, and had no particular smell or taste; they were nearly insoluble in water; solution of potash did not disengage any thing; it did not dissolve them; nor did the mixture give any blue precipitate with sulphate of iron. When heated in a tube into which a piece of paper was introduced moistened with sulphate of iron, the paper became blue, and there was a strong smell of hydrocyanate of ammonia. M. Vauquelin thinks it probable, that in this case, the carbon which is usually deposited from cyanogen during decomposition, had combined with a portion of the undecomposed cyanogen, and thus become insoluble, and precipitating slowly, it had time to combine with a small quantity of water, and assume the crystalline form. M. Vauquelin proposes to call this substance *subcyanogen* or *protocyanogen*.—(Annales de Chimie et de Physique.)

VI. *Preparation of Iodide of Potassium.*

M. Caillot suggests the following method of preparing this compound:—Hydriodate of iron is first formed, and then decomposed by carbonate of potash; for this purpose he takes four parts of iodine, two of bright iron filings, and about twenty of water. These three substances are to be put in a glass or porcelain capsule. The mixture is to be stirred until the liquor which soon becomes of a deep-brown colour, is rendered colourless; the liquor is then made to boil, and a solution of subcarbonate of potash is to be added until precipitation ceases; or a small excess of the alkali may be used, and saturated with hydriodic acid after filtration. The residuum is to be washed till it ceases to afford a precipitate on the addition of permuriate of mercury: the filtered liquors being then mixed, the whole is to be evaporated till a pellicle appears.

The same process may be employed for preparing the iodides of sodium, magnesium, calcium, &c. The iodides of mercury may also be prepared by decomposing the protonitrate and permuriate of mercury by means of hydriodate of iron, which, as just shown, may be formed extemporaneously.—(Annales de Chimie et de Physique.)

VII. *Butter.*

M. Chevreul has lately subjected the butter of cows' milk to examination. He finds that 100 parts of fresh butter consist of

Pure butter	83·75
Butter-milk	16·25

From numerous experiments, M. Chevreul concludes that there exist in the oil of butter at least two fluid substances, one of which is soluble in all proportions in cold alcohol, does not possess acid properties, and gives by saponification some sweet principle, butiric, caproic, capric,

margaric, and oleic acids. M. Chevreul has given this oil the name of *buterin*, because it contains the butiric acid (or its elements), to which butter owes its odour. The other fluid substance has the properties of olein.—(Ann. de Chimie et de Physique.)

VIII. *Carbonate of Magnesia in the Urinary Calculi of Herbivorous Animals.*

M. Lassaigne remarks, that but few of those chemists who have examined the urinary calculi of herbivorous animals have mentioned carbonate of magnesia as one of their constituents; but MM. Wurser, John, and Stromeyer, have discovered its existence; the two first in the urinary calculus of the horse, and the last on a calculus taken from a cow.

The results of M. Chevreul's analysis of the urine of the horse, which he found to contain carbonate of magnesia, induced M. Lassaigne to examine the urinary concretions of the same animal; in which he readily discovered it, as well as in those of the ox and the cow. By treating these calculi with sulphuric acid, sulphate of lime was principally formed, but by subsequent operations magnesia was procured. The quantity of carbonate of magnesia is small, forming only the 150th to the 200th of the weight of the calculus.—(Ann. de Chimie et de Phys.)

IX. *Safety of Steam Engines.*

M. Dupin lately read to the Academy of Sciences, the conclusion of the report which he drew up in the name of a commission, to consider the employment of low and high pressure steam-engines, principally with regard to the safety of the public. The commissioners were MM. Laplace, Prony, Ampere, Girard, and Dupin. M. Gay-Lussac, whose opinions differed in many respects from those adopted in the Report, requested permission to withdraw from the commission.

The recommendations adopted by the majority of the Academy were:

1. To have two safety valves adapted to the boiler; one of these valves being so placed as not to be altered by the workman who has the direction of the steam-engine. The other valve to be under his controul, since he may have occasion to diminish the pressure, whereas he would attempt in vain to increase it, because the valve which he could not alter would suffer the vapour to escape.

2. It is proposed to prove the strength of all the boilers by means of the hydraulic press, by causing them to withstand a pressure four or five times greater than they would be required for the usual working of the machine, as well as that this pressure should be limited to four atmospheres. And also that the proof pressure should as many times exceed that of the usual working pressure of the machine, as the latter does that of the atmosphere.

3. Every manufacturer of steam-engines should be compelled to declare his method of proof, and every circumstance which would tend to guarantee the solidity and safety of the machine, especially of the boiler and its appurtenances. The manufacturer ought also to acquaint those in authority as well as the public, with the pressure under which these machines ought to work.

4. The boilers of those steam-engines which are near any house, to be surrounded with a wall, provided the engines are sufficiently power-

ful in case of accident to destroy the partition wall between the house and the establishment which contains the steam-engine.

The commission also proposes that an exact account should be kept by authority of all the accidents which happen to steam-engines of every construction, and to publish this statement, mentioning the causes and effects of such events, the name of the manufacturer of the steam-engine, and this (they observe) is the most efficacious of all methods to prevent the misfortunes which result from the use of steam-engines, whether of low, middling, or high pressure.—(Ann. de Chim.)

X. *On the Phosphates of Lead.* By N. J. Winch, Esq. Hon. MGS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Newcastle-upon-Tyne, Jan.* 23, 1823.

From an itinerant dealer, who collects minerals at the lead hills in Scotland, I lately procured a *variety* of the phosphate of lead, which I suspect is not described in any of our mineralogical arrangements, or scientific journals. The ore in question is of as bright and deep an orange-red colour as the chromate of lead, and consists of groups of simple six-sided prismatic crystals, from an eighth to a quarter of an inch in length, filling cavities in pale-yellow crystalline phosphate of lead. The crystals are brittle, possess an adamantine lustre, and are accompanied by grey, white, and lemon-coloured carbonates of lead, together with galena. Placed on charcoal before the reducing flame of the blowpipe, it decrepitates, and immediately becomes nearly black; then easily fuses into a pale-grey enamel. On borax being added, it melts with effervescence, and the glass formed is of a yellowish milky hue while cooling, but transparent and colourless when quite cold, with air bubbles and globules of lead dispersed through it. Here it may not be amiss to mention the results obtained by means of the blowpipe by some of the most able writers on mineralogy, on testing this ore. Brongniart, at p. 201, vol. ii. says, "Le plomb phosphaté ne fait aucune effervescence dans les acides, et se fond au chalumeau sur le charbon en un globule qui prend un surface polyédrique en si figeant. Il n'est point reductible en plomb sans l'addition d'un peu potasse et de charbon." Berzelius on the Blowpipe, at p. 158, observes, "Phosphate of lead alone on charcoal fuses in the exterior flame; the globule crystallizes; and, after cooling, has a dark colour. In the interior flame, it exhales the vapour of lead, the flame assumes a bluish colour, and the globule on cooling forms crystals with broad facets inclining to pearly whiteness. At the moment it crystallizes, a gleam of ignition may be perceived in the globule. With borax, it behaves like oxide of lead." Phillips's account of this process, at p. 256, is as follows: "Before the blowpipe on charcoal, phosphate of lead usually decrepitates; then melts, and on cooling forms a polyedral globule, the faces of which present concentric polygons. If this globule be pulverized and mixed with borax, and again heated, a milk-white enamel is the first result. On the continuance of the heat, the globule effervesces, and at length becomes perfectly transparent, the lower part of it being studded with metallic lead." In the third edition of Jameson's Mineralogy, vol. ii. p. 372, the same account is given; but in the second edition, vol. ii. p. 368, that author observes, "Before the blowpipe, phosphate of lead does not fly into pieces, but

becomes white, and melts very easily into a greyish globule, but without being reduced even with charcoal. From my own experiments, I have found, 1. That orange-red phosphate of lead behaves in some respects differently from all the varieties tested by these eminent writers. 2. That minute green crystals from Suvside lead mine in Nitherdale, Yorkshire, gave the same results as detailed by Berzelius and Brongniart, but more particularly by Phillips. 3. That opaque pea-green botryoidal phosphate from Germany, and pale-yellow from the lead hill mines, in the reducing flame first became white, and on a stronger heat being applied, melted into a grey opaque globule. With the addition of borax, it effervesced, burned, and was at length reduced into a glass, milky while cooling, but transparent when cold, and containing small globules of lead. Thus it appears, that the crystallized and botryoidal, the orange-red, pale-yellow, and green phosphates of lead, are variously affected by the action of fire, which leads to the conclusion that different ingredients, as well as ingredients in very different proportions, must enter into the composition of the several varieties of this ore; and in its description, it is not sufficient to mention how any *single variety* behaves under the influence of the blow-pipe. I remain, Sir, your obedient servant,

N. J. Winch.

XI. *Maclureite, or Fluo-silicate of Magnesia, a new Mineral Species from New Jersey.*

This mineral was discovered, several years ago, near Sparta, in Sussex County, New Jersey, by the late Dr. Bruce. It was at first supposed to be *sphene;* but subsequent investigations led to its being ranked with *condrodite,* a mineral discovered in Sweden, and analyzed by M. d'Ohsson, whose results, confirmed by Berzelius, were as follows:

Silica	38·00
Magnesia	54·00
Oxide of iron	5·1
Alumina	1·5
Potassa	0·86
Manganese	Trace
Loss	0·54
	100·00

The new mineral, however, though it resembles *condrodite* in external characters, differs essentially from it in chemical composition, as was proved from an analysis, which appears to have been made with care and skill, by Mr. Henry Seybert, of Philadelphia.

Though the pulverized mineral gives no indication of fluoric acid, when acted upon by an excess of heated sulphuric acid, and though other processes failed to detect it, yet fluoric acid was distinctly traced in the silica, remaining after the calcined mineral had been first boiled with nitromuriatic acid (which converted it into a jelly), and then heated with water acidulated with muriatic acid. The silica, thus obtained, effervesced violently with sulphuric acid, and gave fluosilicic acid in abundance, disengaged, it should appear, from the insoluble compound of potassa, silica, and fluoric acid, described by Gay-Lussac

and Thenard. The constituents of the mineral were determined to be as follows:

Water	1·000
Fluoric acid	4·086
Silica	32·666
Peroxide of iron	2·333
Magnesia	54·000
Potassa	2·108
Loss	3·807
	100·000

(Silliman's American Journal, vol. v. p. 2.)

XII. *Combustion of a Stream of Hydrogen Gas under Water.*

Mr. Thomas Skidmore, of New York, has discovered that if the flame produced by the combustion of hydrogen gas, issuing in combination with oxygen from the compound blowpipe of Dr. Hare, be plunged below the surface of water, it continues notwithstanding its submersion in, and actual contact with, that fluid, *to burn*, apparently with the same splendour as it does in the common air. The only discoverable difference is, that when the flame burns into water, it seems, if the expression may be allowed, to *conglobate* its figure; whereas in the air, it assumes the shape of a long slender conical pencil. Care is required that the flame be introduced slowly and gently into the water, in order to avoid the recession of the flame into the interior of the tube, at its first entrance, which is apt to take place if suddenly immersed. To obviate this evil more effectually, tubes of a fine capillary bore are best adapted.

When a piece of cork or pine wood was applied to the submersed gaseous flame, it gave out a brilliant light, and this appearance continued till the recession took place, which, in some instances, might be for a minute or two. Small pieces of copper wire, 1-40th of an inch diameter, became red-hot when exposed to the flame under water in full day-light. The discoverer of this property of the flame of the compound blowpipe suggests its application to the purpose of a *submarine instrument of naval warfare*, and thinks there are no difficulties in the way of its being so employed that may not be easily overcome. —(American Journal.)

XIII. *Fusion and Volatilization of Charcoal.*

The fusion and evaporation of charcoal has been effected in America with the assistance of Dr. Hare's galvanic deflagrator. Prof. Griscom, of New York, describes the experiment in the following terms: "With a deflagrator, of considerable size and in good order, these experiments are, in fact, extremely easy; and with well prepared charcoal will never fail in a single instance. The surface of the fused charcoal is brilliant, with a metallic and frequently iridescent lustre. Upon the charcoal on the copper side, there is no appearance of fusion, but a crater-shaped cavity extremely well defined, with the proper fibrous and porous appearance of charcoal; every thing indicating that the charcoal is wasted from this pole, and transferred to the other. It seems to pass in the state of vapour, to be accumulated or condensed

on the positive pole, and then to undergo fusion by intense heat. In about three seconds, a decisive result is obtained.

Charcoal, which has been thus fused, is found to have acquired a great increase of specific gravity. It sinks readily in strong sulphuric acid, though common charcoal floats readily in water with at least half its volume out. It is rendered also very difficult of combustion, but may be burned away, leaving no residuum if heated by a powerful lens in a vessel over mercury filled with oxygen gas. The gas produced was ascertained to be pure carbonic acid. Strong sulphuric acid may be boiled without effect on charcoal which has been fused. Even the strongest nitric acid in the cold does not act upon it, and at a boiling temperature, the action is very slight, and ceases the moment the heat is withdrawn.—(American Journal.)

XIV. *Alteration of the freezing Point of Thermometers by being long kept.*

It is asserted (Annales de Chimie et de Physique, Nov. 1822, p. 330), that a thermometer on which the freezing point has been exactly marked, becomes incorrect in process of time, at the end of a year for example, and indicates, when plunged into melting ice, a temperature a little above freezing, as if the bulb had become smaller. This fact, originally observed by Bellani, of Monza, in the Milanese, was confirmed by Pictet's experiments in six different thermometers. In one of these, made 40 years ago, the freezing point had risen to + 0·1 centigrade. M. Flaugergues, the astronomer, after satisfying himself of the fact, has endeavoured to assign a reason for it in the diminishing elasticity of the glass of the thermometric ball, which, like all other springs, loses its force by being kept long in a state of tension."

A correspondent of the Editor of this journal has been induced, by the foregoing notice, to examine several thermometers which he has had for many years; but has not been able to discover the deviation above remarked. Two of these, made by Crichton, of Glasgow, having very small cylindroidal bulbs, have been in his possession nearly twenty years. In these, the freezing point is marked by a file on the stem, and when plunged into thawing snow, not the smallest change is observable in the height at which the mercury now stands. In one or two others, out of ten which were examined, there did appear a little deviation from the freezing point marked upon them; but they had not been constructed by makers of any eminence, and had probably been inaccurate from the first. The change, therefore, though scarcely to be questioned on such testimony, appears not to be universal.

XV. *Excrement of the Boa.*

Prof. Psaff found that the fresh solid excrement of the boa is insoluble in cold water, but dissolved by about 800 times its weight of boiling water. The greater part of what is dissolved is deposited as the water cools, and this deposit is partly pulverulent, and partly on fine shining scales, circumstances which characterise uric acid.

With nitric acid, the general phenomena exhibited by uric acid were also produced, but the Professor observed, that when evaporated with nitric acid to a certain point, and before purpuric acid is formed, the solution deposits a considerable quantity of crystallized nitrate of ammonia; after the first portion of crystals were separated by evaporat-

ing the solution, a further quantity was obtained; when after this, the solution was evaporated to dryness, no purpuric acid was obtained; but, on the contrary, if the solution in nitric acid be immediately evaporated to dryness, purpuric acid is formed.

The excrement of the boa contains ammonia, and in so great excess that it may be considered as a suburate of ammonia; when distilled with a weak solution of potash, water containing ammonia is condensed in the receiver; when the experiment was repeated with uric acid, no ammonia was obtained. When the excrement is burnt, the ashes are found to contain oxide of iron and carbonate of lime, but no phosphate of lime.—(Schweigger's Journal.)

XVI. *Heliotrope.*

According to Dr. Brandes and Firnhaber, the heliotrope is composed of

Silica	96·25
Protoxide of iron	1·25
Alumina	0·86
Water	1·05
	99·41

It, therefore, resembles chalcedony on silica being slightly mixed with other bodies.—(Ibid.)

XVI. *Carbonate of Magnesia and Iron, &c.*

Prof. Walmstadt, of Upsal, has analysed carbonate of magnesia from Hartz containing the carbonates of iron and manganese. The texture of this mineral is foliated, and its primary form is a rhomboid of 108° 15′, differing of course still more from calcareous spar than rhomb spar. The results of the analysis were:

Carbonate of magnesia	84·36
Carbonate of iron	10·02
Carbonate of manganese	3·19
Silica	0·30
Water	0·51
Loss, and a substance destructible by fire	1·62
	100·00

(Ibid.)

XVII. *On the Absence of Carbonic Acid in the Atmosphere over the Sea*

M. Vogel found that atmospheric air taken over the sea half a mile from the sea-shore off Doberan, contained so little carbonic acid, that a solution of pure barytes was hardly made turbid by it; while the same bulk of air taken on shore produced a considerable quantity of carbonate of barytes.

M. Vogel repeated these experiments in 1822 in the Channel, two leagues from Dieppe, where he emptied a large bottle with distilled water, and tried the air afterwards with a solution of pure barytes, which became so little turbid that it hardly could be perceived; when the experiment was repeated on shore, the solution of barytes became extremely

turbid. M. Vogel adds, this may easily be conceived as the animal substances; although they easily putrify and form carbonic acid, cannot communicate it to the air, because the sea-water absorbs it.

XVIII. *Hydriodide of Carbon.*

According to M. Serrulas, hydriodide of carbon may be plentifully obtained by merely treating a solution of iodine in alcohol, with one of caustic soda or potash in the same fluid.—(Ann. de Chimie.)

Article XV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Mr. W. West, of Leeds, is about to publish in a separate form, with additions, his Analysis of the New Sulphur Spring at Harrogate.

Sabæan Researches; in a Series of Essays addressed to distinguished Antiquaries, and including the Substance of a Course of Lectures delivered at the Royal Institution, on the Engraved Hieroglyphics of Chaldæa, Egypt, and Canaan. By John Landseer, FSA. &c. Illustrated by Engravings of Babylonian Cylinders, and other inedited Monuments of Antiquity.

Sir John Malcolm is preparing for the press, a Memoir of Central India, with the History, and copious Illustrations of the past and present State, of that Country, and an original Map.

A Practical Treatise on the various Methods of Heating Buildings by Steam, Hot Air, Stoves, and open Fires; with explanatory Engravings.

Elements of a new Arithmetical Notation, in some respect analogous to that of Decimals, by which Expressions producing a great Variety of Infinite Series may be obtained.

JUST PUBLISHED.

The Encyclopædia Metropolitana, Part IX. containing, under the class of the mixed and applied Sciences, the completion of the article on Physical Astronomy.

Part I. of the 16th volume of The Edinburgh Encyclopædia, conducted by Dr. Brewster, in which, among other articles, are, Orkney Islands, Ornithology, Paper-making, Parallax, Parallel Roads, Partial Differences, Patents, Pearl Fishery, and Pendulum. With 14 Engravings from original Drawings. 1*l.* 5*s.*

Sylva Florifera, the Shrubbery; containing an Historical and Botanical Account of the Flowering, Shrubs, and Trees, which now ornament the Shrubbery, the Park, and Rural Scenes in general; with Observations on the Formation of ornamental Plantations, and picturesque Scenery. 2 Vols. 8vo. 1*l.* 1*s.* Boards.

Article XVI.

NEW PATENTS.

G. E. Harpur and B. Baylis, of Weedon, Northamptonshire, engineers; for a method of impelling machinery.—March 18.

R. Badwell, the younger, of Leek, Staffordshire, silk-manufacturer, for certain improvements in the throwing, twisting, or spinning of sewing-silk, Organzine, Bergam, and such other descriptions of silk as the said improvements may be applicable to.—March 18.

H. H. Price, of Neath Abbey, Glamorganshire, engineer, being one of the people called Quakers, for an apparatus for giving increased effect to paddles used in steam vessels, applicable to rotary movements, by which they are generally worked.—March 18.

W. Crighton and J. Crighton, both of Manchester, Lancashire, machine-makers; for an improvement in the construction of the cylinders used in carding-engines, and other machines employed in the preparation for the spinning of cotton, flax, wool, silk, and mixtures of the said materials or substances.—March 18.

W. Bailey, of High Holborn, Middlesex, ironmonger, and T. Horne, the younger, of Belmont-row, Birmingham, Warwickshire, brass-founder, for improvements in the manufacture of metallic window frames, and other metallic mouldings, applicable to the ornamenting of furniture.—March 18.

T. Rogers, of Buckingham-street, Strand, Middlesex, Esq. for an improvement on stays and bodices which improvement is also applicable to boots.—March 18.

W. Hope, of Jedburgh, Roxburgh, North Britain, ironfounder, for certain improvements in the construction of printing-presses.—March 18.

T. Hancock, of Goswell Mews, Saint Luke, Old-street, Middlesex, patent cork manufacturer, for an improvement in the preparation, for various useful purposes, of pitch and of tar.—March 22.

T. Wickham, of Nottingham, lace-manufacturer, for a compound paste and liquid, for improving and colouring lace and net, and all other manufactured articles made of flax, cotton, wool, silk, or any other animal or vegetable substance.—March 24.

W. Jessop, of Butterley Hall, Derbyshire, ironmaster, for an elastic metallic piston, or packing of pistons, to be applied either externally or internally to cylinders.—March 27.

W. Warcup, of Dartford, Kent, engineer, for an improvement in the construction of a machine called a mangle.—April 3.

J. Frost, of Finchley, Middlesex, builder, for improvements in the process of calcining, and preparing calcareous and other substances, for the purpose of forming cements.—April 3.

C. Pope, of Bristol, spelter-maker and metal-merchant, for a composition of certain metals to be used for the purpose of sheathing the bottoms of ships and vessels, and of roofing the tops of houses, or for any other purpose to which such composition may be applicable.—April 8.

D. W. Acraman, of Bristol, iron-manufacturer, and W. Piper, of the Cookley Ironworks, Worcestershire, iron-manufacturer, for certain improvements in the preparation of iron, for the better manufacture of chains and chain-cables.—April 12.

J. M. Hanchett, of Crescent-place, Blackfriars, for improvements in propelling boats and vessels.—April 12.

J. Francis, Norwich, shawl and bombasin-manufacturer, for an improvement in the process of manufacturing a certain article, composed of silk and worsted, for useful purposes.—April 12.

G. Graulhie, of Castle-street, Holborn, gent. for a machine upon a new and portable construction, capable of being inclined in different degrees, adapted to the conveyance of persons and goods over water or ravines, for military or other objects, and also to purposes of recreation and exercise.—April 16.

J. Johnson, of Waterloo Bridge Wharf, Middlesex, for certain improvements on drags to be used for carriages.—April 16.

S. Hall, of Basford, Nottinghamshire, cotton-spinner, for a certain method of improving lace, net, muslin, and calico.—April 18.

W. Southworth, of Sharples, Lancashire, bleacher, for certain machinery or apparatus adapted to facilitate the operation of drying calicoes, muslins, linens, or other similar fabrics.—April 19.

R. Winter, of Fen-court, Esq. for an improved method of conducting the process of distillation.—April 22.

R. J. Tyers, of Piccadilly, Middlesex, fruiterer, for a machine to be attached to boots, shoes, or other covering of the feet, for the purposes of travelling or pleasure.—April 22.

W. Palmer, of Lothbury, paper-hanger, for certain improvements in machinery, for the purpose of painting or staining paper for paper hangings.—April 22.

F. G. Spilsbury, of Walsall, Staffordshire, for certain improvements in tanning.—April 22.

F. Deakin, of Birmingham, Warwickshire, wire-drawer, for an improved method of manufacturing furniture, and for an improvement to the mounting of umbrellas and parasols.—April 22.

J. Rawlins, of Penton-place, Pentonville, Middlesex, gent. for a bedstead, machine, or apparatus, for the relief of invalids.—April 22.

J. Hall, the younger, of Dartford, Kent, engineer, for an improvement in the machinery to be employed for effecting or producing the pressure on linseed, rapeseed, or any other oleaginous seeds or substances from which oil can be expressed, for the purpose of expressing oil from the aforesaid seeds or substances.—April 22.

J. Taylor, of Manchester, for certain improved machinery to facilitate the operation of spinning, doubling, and throwing silk, cotton, wool, or flax, or mixtures of the said substances.—April 29.

J. Bourdieu, of Lime-street, for a discovery and preparation of a mucilage, or slackening matter, to be used in painting or colouring linen, woollen, and cotton cloths, and silks, in cases in which gums, mucilages, and other thickening matters, are now employed.—April 29.

W. Caslon, the younger, of Burton-crescent, Middlesex, Proprietor of Gas Works, for certain improvements in the construction of gasometers.—May 10.

E. Eyre, of Sheffield, Yorkshire, fender-manufacturer, for an improvement in the manufacture of fenders, of brass, iron, or steel.—May 15.

J. Perkins, of Fleet-street, engineer, for certain improvements in the mode of heating, boiling, or evaporating, by steam, of fluids, in pans, boilers, or other vessels.—May 17.

Article XVII.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
5th Mon.								
May 1	Var.	30·49	30·45	72	35	—		
2	N E	30·45	30·39	74	38	—		
3	E	30·49	30·39	70	37	—		
4	E	30·50	30·27	55	33	—		
5	S	30·27	30·07	71	38	—		
6	E	30·07	29·91	76	48	·89		
7	S W	29·92	29·91	78	50	—		
8	S W	29·91	29·80	65	43	—	03	
9	S W	29·91	29·91	61	51	—		
10	S W	29·91	29·82	65	51	—	27	
11	S W	29·82	29·73	66	52	—	—	
12	S W	29·75	29·73	63	48	·83	02	
13	S W	29·84	29·75	65	40	—		
14	W	30·10	29·84	63	44	—	02	
15	N W	30·19	30·10	67	49	—		
16	S W	30·19	29·98	63	43	—	02	
17	W	30·26	29·98	64	34	—	10	
18	S W	30·26	29·97	67	41	·98		
19	E	29·97	29·79	67	50	—		
20	S	29·79	29·77	70	52	—	12	
21	W	29·80	29·77	67	52	—	02	
22	S W	29·86	29·80	62	50	—	07	
23	S W	29·99	29·86	64	44	—	07	
24	S W	29·99	29·84	67	51	—	—	
25	E	29·84	29·82	68	46	—	02	
26	S	30·04	29·82	72	40	·89	22	
27	N E	30·11	30·04	71	44	—		
28	N E	30·22	30·11	77	42	—		
29	N	30·22	30·26	72	41	—		
30	S E	30·29	30·26	78	43	—		
31	E	30·29	30·25	77	51	·82		
		30·50	29·73	78	33	4·41	·98	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fifth Month.—1, 2, 3. Fine. 4. Fine: very cold wind. 5, 6. Fine. 7. Fine, with occasional clouds. 8. Cloudy morning: cold wind. 9. Cloudy. 10. Cloudy: rainy evening. 11. Cloudy: wind boisterous. 12. Cloudy. 13—15. Fine. 16. Cloudy: some rain at nine, a. m. 17. Showery. 18—21. Fine. 22—26. Showery. 27—31. Fine.

RESULTS.

Winds: N, 1; NE, 3; E, 6; SE, 1; S, 3; SW, 12; W, 3; NW, 1; Var. 1.

Barometer: Mean height

For the month.. 30·034 inches.

For the lunar period, ending the 3d .... 30·040

For 15 days, ending the 6th (moon south) .... 30·219

For 12 days, ending the 18th (moon north) .... 29·945

Thermometer: Mean height

For the month.. 56·419°

For the lunar period, ending the 3d .... 45·650

For 31 days, the sun in Taurus .... 52·338

Evaporation.. 4·41 in.

Rain. .... 0·98

Laboratory, Stratford, Sixth Month, 23, 1823. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1823.

Article I.

New Experiments on Sound. By Mr. C. Wheatstone.

(To the Editor of the *Annals of Philosophy.*)

On the Phonic Molecular Vibrations.

SIR,

Before I enter on the immediate subject of this article, it may be necessary to exhibit a general view of those bodies, which, being properly excited, make those sensible oscillations, which have been thought to be the proximate causes of all the phenomena of sound. These bodies, to avoid many circumlocutions otherwise inevitable, I have termed Phonics.

Linear Phonics.

Transversal,	*Longitudinal,*
Making their oscillations at right angles to their axis.	Making their oscillations in the direction of their axis.
1. Capable of tension, or variable rigidity: chords, or wires.	1. Columns of aeriform fluids or liquids: cylindric and prismatic rods.
2. Permanently rigid: rods, forks, rings, &c.	

Superficial Phonics.

1. Capable of tension: extended membranes.
2. Permanently rigid; laminæ, bells, vases, &c.

Solid Phonics.

1. Volumes of aeriform fluids.

The sensation of sound can be excited by any of these bodies when they oscillate sufficiently rapidly, either entire, or divided into any number of parts in equilibrium with each other. The laws of these subdivisions differ in the various phonics according to their form and mode of connection or insulation; and the velocities of the oscillations, or degrees of tune, depend on the form, dimensions, mode of connection, mode of division, and elasticity of the body employed. The points of division in linear phonics are called nodes, and the boundaries of the vibrating parts of elastic surfaces are termed nodal lines. The parts at which the oscillatory portions have their greatest excursions are named centres of vibration; these are always at the greatest mean distances from the nodal points or lines.

These mechanical oscillations are not, however, themselves the immediate causes of sound; they are but the agents in producing in the bodies themselves, and in other contiguous substances, isochronous vibrations of certain particles varying in magnitude according to the degree of tune. I convinced myself of this important fact by the following simple experiments: I took a plate of glass capable of vibrating in several different modes, and covered it with a layer of water; on causing it to vibrate by the action of a bow, a beautiful reticulated surface of vibrating particles commenced at the centres of the vibrating parts, and increased in dimensions as the excursions were made larger. When a more acute sound was produced, the centres consequently became more numerous, and the number of coexisting vibrating particles likewise increased, but their magnitudes proportionably diminished. The sounds of elastic laminæ are generally supposed to be owing to the entire oscillations of the simple parts as shown by Chladni, when, by strewing sand over the sonorous plates, he observed the particles repulsed by the vibrating parts, accumulate on the nodal lines, and indicate the bounds of the sensible oscillations. Did no other motions exist in the plate but these entire oscillations, the water laid on its surface would, on account of its cohesion to the glass, show no peculiar phenomena, but the appearances above described clearly demonstrate that the oscillating parts consist of a number of vibrating particles of equal magnitudes, the excursions of which are greatest at the centres of vibration, and gradually become less as they recede further from it, until they become almost null at the nodal lines.

To multiply these surfaces, and to observe whether the magnitudes of these particles vary in different media, in a glass vessel of a cylindric form, I superposed three immiscible fluids of different densities; namely, mercury, water, and oil. On producing the sounds corresponding with each mode of division, I observed a number of vibrating parts, agreeing with the sound, and showing similar appearances to the plate, formed on the surfaces of each of the fluids; not the least agitation appeared

in the uniform parts. I afterwards inserted this glass in another vessel of water in order to observe the vibrations of the external surface, and found the same results as in the interior, though the levels of the surfaces were different.

The most accurate method to observe these phenomena is by employing a metallic plate of small dimensions, which must be fixed horizontally in a vice at one end, and covered on its upper side with a surface of water; on causing it to oscillate entirely by means of a bow, a regular succession of these vibrating corpuscles will appear arranged parallel to the two directions of the plate, and if the action of the bow be rendered continuous, their absolute number might be counted with the aid of a micrometer. Diminishing the oscillating part of the plate to one half of its length, the double octave to the preceding was heard, agreeably to the established rule, that the velocities of the oscillations are inversely as the squares of the lengths; four vibrating corpuscles then occupied the space before occupied by one, and the absolute number was double to that in the former instance; but the absolute number of these corpuscles have no influence whatever on the degree of tune, which entirely depends on their relative magnitude in the same substance; theory shows us that in plates of this description alteration of breadth does not affect the degree of tune; let us, therefore, reduce this half of the plate to half its breadth, and we shall find the note remain the same, but the absolute number of the corpuscles will in this case be equal to that in the entire plate. Let us now take two plates of equal lengths and breadths, but one double in thickness to the other; the rule is, that the velocities of the oscillations are as the thicknesses of the plates; we shall, therefore, in the thicker plate see a double number of particles to that of the other, occupying the same extent of surface. The last circumstance in which two plates may differ is their specific rigidity, and in this respect it will be found that two plates of exactly equal dimensions, and covered with the same number of vibrating corpuscles of equal magnitudes, but of different substances, differ in sound; therefore, the absolute magnitudes of the particles cannot be assumed as a standard of tune, unless regulated by the specific rigidity.

Unassisted by any means of actual admeasurement, the above are but the proximate results sensible to the eye; more extended and accurate experiments are necessary to confirm the results with mathematical certainty. As the absolute magnitudes of these particles will, I imagine, be hereafter a most useful element for calculation, I will here indicate the most effectual way I am acquainted with to arrive at this knowledge. A thick metallic slip of considerable length and breadth, bent similarly to a tuning fork, and fixed at its curved part in a vice, is very easily excited by friction, and a more considerable surface of regularly arranged vibrating particles is seen than in most other superficies; any description of common exciter may be employed,

When this bent plate is excited by percussion, the particles, before their disappearance, will assume an apparent rotatory motion, on account of the force exerted, and its susceptibility of continuing the vibrations. Employing a parallelopedal rod, the appearances of the higher modes of subdivisions are particularly neat; the entire vibrating parts between the nodes form ellipses, and the semi-part at the free end, a regular half of the same figure. It is important to remark, that the crispations of the water only appear on the sides in the plane of oscillation; the other two sides, on one of which the exciter must be applied, do not show similar appearances.

I have also rendered the phonic molecular vibrations visible, when produced by the longitudinal oscillations of a column of air; the following were the means employed: I placed the open end of the head of a flute or flagiolet on the surface of a vessel of water, and on blowing to produce the sound, I observed similar crispations to those described above, forming a circle round the end of the tube, and afterwards appearing to radiate in right lines; on the harmonics of the tube being sounded, the crispations were correspondently diminished in magnitude. These phenomena will be more evident if the tube be raised a little from the surface of the liquid and a thin connecting film be left surrounding it; the vibrating particles will then occupy a greater space, and be more sensible.

The existence of the molecular vibrations being now completely established, it becomes a critical question, in what manner the sensible oscillations induce these vibrating particles. I do not know whether what I am now going to adduce will be admitted as the right explanation, but it is certainly analogous, so far as the superficial and transversal linear oscillations are concerned. A flexible surface, covered with a coat of resinous varnish, being made to assume any curve, the cohesion of the varnish will be destroyed in certain parts, and a number of cracks will be observed more regularly disposed as the force inducing the curve has been more regularly applied; when the original position of the surface is restored, the cracks will be imperceptible, but will again appear at every subsequent motion. Be this as it may, these particles are invariable concomitants of the sensible oscillations, and there is no reason to suppose otherwise than that their vibrations are isochronous with them. To avoid confusion, I have restricted the word vibrations to the motions of the more minute parts, and the term oscillations to those of the sensible divisions. We may reasonably suppose that the molecular vibrations pervade the entire substance of a phonic; their excursions, however, are not the same in all parts, and they can only be rendered visible, when these excursions are large; they may be so few in number as to be entirely inaudible, as in their transmission through linear conductors; but however few, when they are properly directed, they induce the mechani-

cal divisions of sonorous bodies, each of which will give birth to numerous vibrating corpuscles whose excursions are greater, and the sound will be rendered audible. Dr. Savart has well investigated the modes of division in surfaces put in motion by communicated vibrations. All those phonics whose limited superficies preclude them from exciting in themselves a sufficient number of vibrating corpuscles, when insolated, produce scarcely any perceptible sound, as extended chords, tuning forks, &c. but those whose superficies or solidities are more extended, as bells, elastic laminæ, columns of air, &c. produce sufficient volume of sound without accessory means.

Loudness of sound is dependent on the excursions of the vibrations; volume, or fulness of sound, on the number of co-existing particles put in motion. Thus the tones of the Æolian harp, on account of the number of subdivisions of the strings, are remarkably beautiful and rich, without possessing much power; and the sounds of an Harmonica glass, in which a greater number of particles are excited than by any other means, are extraordinarily so united, according to the method of excitation, with considerable intensity; their pervading nature is one of the greatest peculiarities of these sounds.

The following is a recapitulation of the various properties of sound, which are attributable to modifications of the vibrating corpuscles:

The tune	Depends on the	velocities of the vibrations.
The time		continuance of the vibrations.
The intensity		excursions of the vibrations.
The richness, or volume		number of co-existing vibrations.
The quantity (timbre)		magnitudes of the vibrating corpuscles.

It has often been thought necessary to admit the existence of more minute motions than the sensible oscillations, in order to account for many phenomena in the production of sound. Perrault in his "Essai du Bruit," insisted on their necessity more than any other author I have read: he imagined, that the vibrations have a much greater velocity than the oscillations which cause them, but the experiment he adduced to prove this is far from conclusive; he mistook for these vibrations the oscillations of the subdivisions of the long string he employed. Other distinguished philosophers have had ideas of a similar nature, and Chladni thinks their existence necessary to account for the varieties of quality. I, however, conceived I was the first who had indicated these phenomena by experiment, until a few days ago repeating them, together with the others which form the subject of this paper, in the presence of Prof. Oersted, of Copenhagen, he acquainted me with some similar experiments of his own. Substituting a very fine powder, Lycopodion, instead of the sand used by Chladni, for showing the oscillations of

elastic plates, this eminent philosopher found the particles not only repulsed to the nodal lines, but at the same time accumulated in small parcels, on and near the centres of vibration; these appearances he presumed to indicate more minute vibrations, which were the causes of the quality of the sound: subsequently he confirmed his opinion, by observing the crispations of water, or alcohol, on similar plates, and showed that the same minute vibrations must take place in the transmitting medium, as they were equally produced in a surface of water, when the sounding plate was dipped into a mass of this fluid. These experiments were inserted in Lieber's History of Natural Philosophy, 1813.

Rectilineal Transmission of Sound.

As the laws of the communication of the phonic vibrations are more evident in linear conductors, I shall confine the present article to a summary of their principal phenomena.

In my first experiments on this subject, I placed a tuning fork, or a chord extended on a bow, on the extremity of a glass, or metallic rod, five feet in length, communicating with a sounding board; the sound was heard as instantaneously as when the fork was in immediate contact, and it immediately ceased when the rod was removed from the sounding board, or the fork from the rod. From this it is evident that the vibrations, inaudible in their transmission, being multiplied by meeting with a sonorous body, become very sensibly heard. Pursuing my investigations on this subject, I have discovered means for transmitting, through rods of much greater lengths and of very inconsiderable thicknesses, the sounds of all musical instruments dependant on the vibrations of solid bodies, and of many descriptions of wind instruments. It is astonishing how all the varieties of tune, quality, and audibility, and all the combinations of harmony, are thus transmitted unimpaired, and again rendered audible by communication with an appropriate receiver. One of the practical applications of this discovery has been exhibited in London for about two years under the appellation of "The Enchanted Lyre." So perfect was the illusion in this instance from the intense vibratory state of the reciprocating instrument, and from the interception of the sounds of the distant exciting one, that it was universally imagined to be one of the highest efforts of ingenuity in musical mechanism. The details of the extensive modifications of which this invention is susceptible, I shall reserve for a future communication; the external appearance and effects of the individual application above-mentioned have been described in the principal periodical journals.

The transmission of the vibrations through any communicating medium as well as through linear conductors is attended by peculiar phenomena; pulses are formed similar to those in longitudinal phonics, and consequently the centres of vibration and

the nodes are reproduced periodically at equal distances; in this we observe an analogous disposition with regard to light. I had intended to include in this paper all the analogical facts I have observed illustratory of the identity of the causes of these two principal objects of sensation, but want of time, and the danger of delay, now the subject is occupying so much the attention of the scientific world, has induced me hastily to collect the present experiments, and to defer the others for a future opportunity.

The thicknesses of conductors materially influence the power of transmission, and there is a limit of thickness, differing for the different degrees of tune, beyond which the vibrations will not be transmitted. The vibrations of acute sounds can be transmitted through smaller wires than those of grave sounds: a proof of this is easy; attach a tuning fork to one end of a very small wire, and apply the other end to the ear, or a sounding board; on striking the fork rather hard, two co-existing sounds will be produced, that which is more acute will be distinctly heard, but the other will not be transmitted. If the vibrations of a tuning fork be conducted through a piece of brass wire of the size and thickness of a large needle, the sound, imperfectly transmitted, will become more audible by the pressure of the fingers on the conducting wire; but if a steel wire of the same length and thickness be employed, the sound will be unaltered by any pressure, because steel has a greater specific elasticity than brass.

Polarization of Sound.

Hitherto I have only considered the vibrations in their rectilineal transmission; I shall now demonstrate, that they are peculiarly affected, when they pass through conductors bent in different angles. I connected a tuning fork with one extremity of a straight conducting rod, the other end of which communicated with a sounding board; on causing the tuning fork to sound, the vibrations were powerfully transmitted, as might be expected from what has already been explained; but on gradually bending the rod, the sound progressively decreased, and was scarcely perceptible when the angle became a right one; as the angle was made more acute, the phenomena were produced in an inverted order; the intensity gradually increased as it had before diminished, and when the two parts were nearly parallel, it became as powerful as in the rectilineal transmission. By multiplying the right angles in a rod, the transmission of the vibrations may be completely stopped.

To produce these phenomena, however, it is necessary that the axis of the oscillations of the tuning fork should be perpendicular to the plane of the moveable angle, for if they be parallel with it, they will be still considerably transmitted. The following experiment will prove this: I placed a tuning fork perpendi-

cularly on the side of a rectilinear rod; the vibrations were, therefore, communicated at right angles; when the axis of the oscillations of the fork coincided with the rod, the intensity of the transmitted vibrations was at its maximum; in proportion as the axis deviated from parallelium, the intensity of the transmitted vibrations diminished; and, lastly, when it became perpendicular, the intensity was at its minimum. In the second quadrant, the order of the phenomena was inverted as in the former experiment, and a second maximum of intensity took place when the axis of the oscillations had described a semicircumference, and had again become parallel, but in an opposite direction. When the revolution was continued, the intensity of the transmitted vibrations was varied in a similar manner, it progressively diminished as the axis of the oscillations deviated from being parallel with the rod, became the least possible when it arrived at the perpendicular, and again augmented until it remained at its first maximum, which completed its entire revolution.

The phenomena of polarization may be observed in many corded instruments: the cords of the harp are attached at one extremity to a conductor which has the same direction as the sounding board; if any cord be altered from its quiescent position, so that its axis of oscillation shall be parallel with the bridge, or conductor, its tone will be full; but if the oscillations be excited so that their axis shall be at right angles with the conductor, its tone will be feeble. By tuning two adjacent strings of the harp-unisons with each other, the differences of force will be sensible to the eye in the oscillations of the reciprocating string according to the direction in which the other is excited.

It now remains to explain the nature of the vibrations which produce the phenomena, the existence of which has been proved by the preceding experiments. The vibrations generally assume the same direction as the oscillations which induce them; in a longitudinal phonic the vibrations are parallel to its axis; in a transversal phonic, they are perpendicular to this direction; a circular or an elliptic form can be also given to the vibrations by causing the oscillations to assume the same forms. Any vibrating corpuscle can induce isochronous vibrations of similar contiguous corpuscles *in the same plane* either parallel with, or perpendicular to, the direction of the original vibrations, and the polarization of the vibrations consists in the similarity of their directions, by which they propagate themselves equally in the same plane; therefore the vibrations being transmitted through linear conductors, it is the plane in which the vibrations are made that determines their transmission, or non-transmission, when the direction is altered. A longitudinal or a transversal vibration may be transmitted two ways to a conductor bent at right angles; their axis may be in that direction, as to be in the

same plane with the right angle, in which case the former will be transversally, or the latter longitudinally transmitted in the new direction; or their axis may be perpendicular to the plane of this new direction, under which circumstances neither can be communicated.* In explaining the polarization of light, there is no necessity to suppose that the reflecting surfaces act on the luminous vibrations by any actual attracting or repulsing force, causing them to change their axes of vibrations; the directions of the vibrations in different planes, as I have proved exist in the communication of sound, is sufficient to explain every phenomenon relative to the polarization of light.

Let us suppose a number of tuning forks oscillating in different planes, and communicating with one conducting rod; if the rod be rectilinear, all the vibrations will be transmitted, but if it be bent at right angles, they will undergo only a partial transmission; those vibrations whose planes are perpendicular, or nearly so, to the plane of the new direction, will be destroyed. The vibrations are thus completely polarized in one direction, while passing through the new path, and on meeting with a new right angle, they will be transmitted or not, accordingly as the plane of the angle is parallel with, or perpendicular to, the axes of the vibrations. In this point of view, the circumstances attending the phenomena are precisely the same as in the elementary experiment of Malus on the polarization of light.

Double refraction is a consequence of the laws of polarization, by which a combination of vibrations having their axes in different planes, after travelling in the same direction, are separated into two other directions, each polarized in one plane only. That this well-known property of light has a correspondent in the communication of phonic vibrations, I shall now demonstrate. When two tuning forks, sounding different notes by a constant exciter, and making their oscillations perpendicularly to each other, have their vibrations transmitted at the same time through one rod, at the opposite extremity of which two other conductors are attached at right angles, and when each of these conductors is parallel with one of the axes of the oscillations of the forks, on connecting a sounding board with either conductor, those vibrations only will be transmitted through it which are polarized in the same plane with the angle made by the two rods through which the vibrations pass; either sound may be thus

* I have just seen a paper by M. Fresnel, entitled "Considerations Mécaniques sur la Polarization de la Lumiere," in which this eminent philosopher had previously arrived at the same conclusions with respect to light, as I have proved in this communication respecting sound. The important discoveries of Dr. Thomas Young, followed by those of M. Fresnel, have recently re-established the vibratory theory of light, and new facts are every day augmenting its probability. The new views in acoustical science, which I have opened in this paper, will, I presume, give additional confirmation to the opinions of these eminent philosophers; and I hope, when I resume the subject, to be enabled to account for the principal phenomena of coloration, with regard to their acoustic analogies, in a way calculated to establish the permanent validity of the theory.

separately heard, or they may both be heard in combination by connecting both the conductors with sounding boards.

The phenomena of diffraction regarding only the form of the surfaces, or the superficies over which the vibrations extend, are by the conformation of the organs of hearing, not of any consequence to the perception of sound, though the same phenomena when the chromatic vibrations are concerned, are very evident to the eye. They, however, undoubtedly take place equally in both instances, and may be well explained by the theory already laid down. Each separate vibration propagating itself in the plane of its vibrating axis, a number of vibrations in different planes, after passing through an aperture, naturally expand themselves transversely as well as rectilineally, and thereby occupy a greater space than they would, were they only longitudinally transmitted.

I have still to indicate a new property of the phonic vibrations, but whether it is analogous to any of the observed phenomena of light, I am yet ignorant. When the source of the vibrations is in progressive motion, the vibrations emanating from it are transmitted, when the conductor is rectilineal and parallel with the original direction, and they are destroyed when the conductor is perpendicular to the direction, though the axis of vibration and the conductor, being in both instances *in the same place*, would transmit the vibrations were the phonic stationary. These circumstances are proved by the following experiments: When a tuning fork placed perpendicularly to a rod, communicating at one or both extremities with sounding boards, and caused to oscillate with its vibrating axis parallel with the rod, moves along the rod, preserving at the same time its perpendicularity and parallelism, the vibrations will not be transmitted while the movement continues, but the transmission will take place immediately after it has remained motionless. When the tuning fork moves on the upper edge of a plane perpendicular to a sounding board, the vibrations rectilineally transmitted will not be influenced by the progressive motion.

Article II.

On Granite Veins. By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy.*)

Dear Sir, *Helston, May* 7, 1823.

Various statements and representations, have from time to time been given, of the gigantic granite veins which are so very conspicuous in the slate cliffs about a quarter of a mile east of

Fig. 1.

Fig. 2.

Fig. 5.

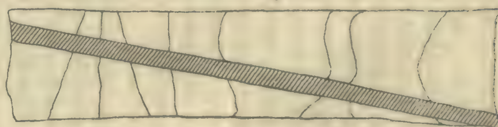


Fig. 3.

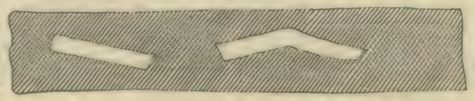
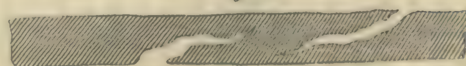


Fig. 4.



 The parts thus delineated are the Granite.

Trewavas Head, in the parish of Breage, in Cornwall, few of which, in my opinion, can be clearly understood by those who have never visited the spot, consequently less likely are they to be able to decide on their disputed nature, whether the granite composing the veins is of the primitive or secondary formation. Having very recently visited the spot, and taken Mr. Sedgwick's description of these veins with me, I find the account given by him nearly correct; at the same time I discover that he has omitted to notice some circumstances which might tend to elucidate, in a more correct manner, the nature of their formation. In endeavouring to supply this deficiency, I have thought it advisable to give a section of the cliff, or an outline of its appearance from the beach at low water (Pl. XXI), fig. 1, and add a few observations which I conceive necessary as we proceed in his description.

"About a quarter of a mile east of Trewavas Point (and about 100 yards east of this sketch), where the cliffs are in an unusually ruinous state, a small brook has excavated a passage to the water's edge. The killas rocks on the beach appear to be intersected by numerous contemporaneous veins of quartz. Near this spot several thin beds of granite seem to alternate with the slate; one in particular, which preserves its thickness and conformity to the lamina of the schist for upwards of 100 feet, when it is lost in the waters." The slate lying both above and below this granitic vein as it traverses the beach, is washed from its surfaces, so as to leave it projecting in many places several feet, so that its dip is very visible, and is found to be as in the cliff at about an angle of 28°. "However, a further examination," says Mr. Sedgwick, "discovered its real nature; for upon observing it in an opposite direction, a number of smaller veins were seen emanating from it. It then cut obliquely through the lamina of slate, starting off from its first direction, and became finally lost in a waving line among the cliffs. The greatest width of this vein is about two feet, and its extent from the edge of the water to its termination in the cliff is about 400 feet.

"Further west, the granite veins are crossed by two others of a different character; one of them ranges nearly in the magnetic meridian, and underlies east two feet in a fathom; the other underlies in an opposite direction. They are about a foot and half in width, and contain quartz, oxide of iron, and a little clay slate." This quartz vein ranging nearly in the magnetic meridian, produces upon the granite vein, the same effect that cross courses often have upon metalliferous veins in most of our mines, that of heaving it out of its direct course. Here, fig. 2, the granite vein is heaved up about three feet by being intersected by the quartz vein; while another quartz vein, a few feet distant, is seen pursuing its regular course, being interrupted only by the granite vein. This circumstance I shall have occasion to remark more fully hereafter.

"For a considerable extent beyond this point, the whole base of the cliffs is covered with vast fragments of the veins which have been denuded by the surrounded killas becoming decomposed; one of these is 10 feet thick. In general they are of a brilliant white colour, and of a fine granular texture, sometimes containing within themselves parallel veins composed of large crystals of quartz and felspar, and proved to be of contemporaneous origin by the long spiculæ of schorl which pass without interruption, through both the quartz and felspar."

These coarse granitic veins within the granite are best seen in many of the huge blocks on the beach; one block in particular, I observed, that has one of its sides nine feet long, and seven broad, covered with these immense crystals of quartz and felspar, and which most probably had separated from its fellow, by the fall from the cliff. One crystal of felspar I separated which measured $4\frac{1}{4}$ inches in diameter These coarse veins, generally speaking, are not more than from four to eight inches thick; but much of the granite *apparently* forming the matrix of the beach, in this place, seem to be wholly composed of these large crystals, in which is found some schorl, and scarcely any mica; while other parts of the granite have merely the large felspar crystals imbedded in it, as to render it completely porphyritic. One block of considerable magnitude has a vein of deep coloured amethyst passing through it, several small crystals of which I collected.

"Beyond the ruin of these veins, there is a bed of granite one foot thick, and about 40 feet in length and breadth." This is the coarse-grained granite just alluded to, but it varies in thickness from one to five feet. This "passes under the cliff, and to all appearance alternating with the slate, but which, as in the former instance, turns out to be a granitic vein. Advancing further to the west, the rocks are beautifully intersected with veins of the like nature, the lower part being cut through by a well defined vein of about a foot thick, while the higher parts are traversed by innumerable ramifications; the lower branch after keeping the direction of the slate beds, for a distance of 60 feet, suddenly rises in a perpendicular direction to the top of the cliff. The whole of this system of veins afterwards unite in one trunk, which after traversing a projecting ledge of rocks, descends in an oblique direction into a great mass of granite, which form a part of a natural cavern. Near this spot appears a very large mass of granite, which seem to be the root of the gigantic veins, which proceed from this point, and rise in broad white lines towards that part of the cliff which reposes immediately on the central granite. Splinters of clay slate are here seen imbedded in the middle of the granitic veins.

"From this point two large veins separated by a lancet-shaped mass of slate, rise towards the west at an angle of about

15°. Within a few feet of these two, a third vein starts out at nearly the same angle, and proceeds in the same direction. These three veins are throughout nearly of the same thickness, viz. each about five feet."

Whether the recent fall from the cliff during the last winter has altered the features of the veins, or exposed a new one, I cannot state with certainty; but there is distinctly to be seen at present four separate veins as represented in the section; the lowest is not more than three feet thick, until it arrives at the point (*a*), when it suddenly widens to more than six feet, at which thickness it continues on to the west. The vein immediately above this (*b*) commences about five feet in thickness, and continues on at the same width as far as immediately over the widened part of the vein below, where it decreases to about $2\frac{1}{2}$ feet, and so continues on to the recess. These two are at about an angle of 15°. The next vein (*c*) is about seven feet thick throughout, and rises at about an angle of 30°, and it is in this vein principally where the fragments of slate are so very conspicuous: some of these fragments measured from three to four feet in length, and from four to six inches thick: they show themselves in the veins in the manner represented in fig. 3. At other places the slate may be seen apparently shooting into the veins of granite in a tortuous manner, fig. 4: a fourth vein (*d*), about eight feet thick, is found rising at an angle of about 45° till it is lost in the alluvial soil above. This vein does not appear to be noticed by Mr. Sedgwick.

On examining some of the rocks lately fallen, many fragments of slate are to be found imbedded in the granite, and several masses of slate may be seen with granite adhering to one or more of its sides, and so firmly attached to it, that the granitic vein itself has split in preference to separating from the slate.

"The two lowest veins preserve their course without being much deflected for some hundred feet, and from the place we first remarked them, disappear behind a projecting part of the cliff. On turning this projecting ledge, we suddenly reached a recess, the lower part of which was filled with the ruins from the higher of the overhanging rocks. The western side of this recess is composed of killas, intersected by some small granitic veins." About half of the western side only is composed of killas; close to the alluvial soil is granite 15 feet thick; then comes a thin layer of slate about three feet thick, which is again followed by a granitic vein (*g*), about 15 feet in thickness. The remaining part of the cliff below is all slate, which entirely disappears about 200 feet further west than this recess. In this last described slate are to be seen blocks of rounded granite, or what in other situations would be called bowlders of small dimensions imbedded in the centre of the slate as seen at (*e*). This granite has a different aspect from any other in the immediate neighbourhood, being of a darker and firmer texture, and

containing its usual quantity of mica; whereas what composes the veins has always a slaty fracture, contains little or no mica, and has a white chalky appearance.

"A protruding mass of granite from the base of the eastern side of this recess to the height of 25 or 30 feet (*f*). It is of a very singular outline, yet does not appear to have shown the slaty laminæ reposing on it out of their usual direction." This I should also denominate a granitic vein, which soon becomes hid and lost on the beach from the ruins of the cliff above. It has in every respect the same characters as the granite of the other veins; by careful examination the slate may be observed beneath the vein, making it about 18 feet thick. Its other end soon becomes lost behind the mound of rubbish in the recess, and from its inclination, I should think the vein (*g*) on the western side its continuation. "The mound of rubbish in the recess enabled us to ascend more than half way up the cliff, and trace the two large veins before mentioned into an enormous bunch of granite, which here reposes on the top of the cliff, and is supported by undisturbed beds of slate; the line of demarcation being nearly horizontal, and at an elevation of 60 or 70 feet above the level of the beach. The denuded face of this bunch of granite is 30 or 40 feet thick. Two or three veins appear to take their origin from this anomalous overlying mass. One spreads out in minute ramifications towards the part of the cliffs which abuts towards Trewavas Point, at the termination of the killas in that direction. Two others descend obliquely, and are lost behind the large mound of rubbish before mentioned."

The whole of the slate has an evident inclination to the east at an angle of about 15°; and in no part of it traversed by the granitic veins, are its laminæ, &c. interrupted. There are evident symptoms of these veins being formed subsequently to the slate; for in one part of the vein (*c*), there is a slight fissure running perpendicular through the slate until it meets the vein, which fissure may be again seen on the opposite side of the vein holding its direct course. Several small quartz veins traverse the slate in all directions, but observe the same law as regards the granitic vein; and in no part whatever could I find either fissure or quartz vein of the slate to penetrate the granite (except the one before mentioned, where the granitic vein is heaved by it); but in every instance to present themselves as in fig. 5. The slate does not make its appearance more than 200 feet west of this recess.

I am, dear Sir, your humble servant,

M. P. Moyle.

Article III.

An Abridged Translation of M. Ramond's Instructions for the Application of the Barometer to the Measurement of Heights, with a Selection from his Tables for facilitating those Operations, reduced (where necessary) to English Measures. By Baden Bowell, MA. of Oriel College, Oxford.

(To the Editor of the *Annals of Philosophy.*)

SIR,

The dissertations and tables of M. Ramond are of such acknowledged excellence for the purposes of the barometrical observer, that I trust the following abstract of them brought into a form more convenient to the English student will not be unacceptable. On a careful perusal of his publication, it appeared to me that the valuable information contained in it was very susceptible of being reduced into a smaller compass; and that among the various tables he has given, those of more essential use might be selected, and, as far as requisite, reduced to English measures. In this way I conceive the most valuable materials of the author may be very usefully collected; and within the compass of three, or at most four papers of such length as is proportionate to the size of a number of the *Annals*, I trust I shall be able to present the scientific inquirer with a compendium of much information highly requisite to be attended to in the measurement of heights by the barometer, and with a set of tables which seem to unite facility of operation with correctness of result, in a greater degree than any extant. B. P.

General Principles of Barometrical Measurement.

It is well known that in the barometer the mercury sinks as we are elevated above the level of the sea; this indeed must be the case, for the barometer may be considered as a balance in which the column of mercury keeps in equilibrio with the corresponding column of air. At the level of the sea, it balances the whole weight of the atmosphere: at a greater elevation, only a part of it. The quantity by which it has sunk expresses the weight of the stratum of air intercepted between the levels of the two stations. Considered in relation to the measure of height, it expresses the difference of level in a ratio depending on that of the densities of mercury and air. What then is the thickness of the stratum of air whose weight is equal to that of an inch of mercury? To such a question may the problem of the mensuration of heights by the barometer be ultimately reduced. This question, however, apparently so simple, has nevertheless occasioned much difficulty to philosophers.

If the air were, like mercury, an incompressible fluid and of uniform density, the solution of the problem would not have presented any difficulty. It would then have sufficed to establish once for all the ratio of the densities in order to infer that of the volumes, and to determine the thickness of the stratum of air whose weight was in equilibrio with a given column of mercury of the same diameter.

But air is elastic; it dilates or condenses in proportion to the pressure it undergoes; and in proportion as we rise in the atmosphere, we perceive its density diminish along with the weight by which it is compressed. If then we suppose a column of air divided into strata of equal thickness, these strata beginning from below will diminish gradually in weight, and will correspond respectively to portions of the mercurial column gradually smaller: in such a manner that *equal* differences of elevation will be marked in the barometer by successive depressions of the mercury so much the *smaller* as we rise higher.

We perceive then that in a column of air supposed at a uniform temperature, the density of the strata decreases in proportion as the compressing weight diminishes, which is represented by the height of the column of mercury. Setting out from this first datum, and imagining the column of air divided into strata bounded by planes indefinitely near each other, we are led to perceive that the differential variation of the density is proportional to the product of this density multiplied into the variation in vertical height. And if we make this height vary by quantities constantly equal, the ratio of the differential of the density to the density itself will be constant, which is the characteristic property of a decreasing geometrical progression whose terms approach indefinitely near to each other.* Hence it follows, that if the heights of the strata increase in arithmetical progression, their density, and consequently their weight, and consequently also the heights of the barometer will decrease in geometrical progression. This law is the fundamental principle of the application of the barometer to the measurement of heights.

Long before philosophers were aware of it, there existed a book which seemed made expressly for facilitating the application of this principle. The logarithmic tables, the admirable artifice of which had already so much abridged the long calculations of astronomy, offered a double series of corresponding numbers, one of which proceeded in arithmetical, the other in geometrical progression; numbers, which even the most courageous patience would doubtless never have had resolution enough to calculate solely for the sake of the measurement of heights, even if, in other respects, this art, as yet in its infancy, had been capable of suggesting the idea. It required some genius even to

* Exposit. du Syst. du Monde. Third Edit. tom. i. p. 155.

conceive this new application of tables hitherto signalized by so many services of a totally different kind. The name of Mariotte remains coupled with a happy approximation, which seemed as if it ought to have been made at once by every one, but which he himself did not turn to any advantage. We know, however, that the heights of the barometer at the two stations being expressed whether in inches or any other measure, the difference of level is represented by the difference of the logarithms of these heights.

But this representation is only an abstract one; it indicates a ratio, and not absolute measures, because the system of the tables is not framed in the particular system belonging to the measures of heights.

In order that the difference of the logarithms may be transformed into feet, we must apply to it in a particular manner the value corresponding to these measures,* combined with the ratio of the densities of mercury and air. These conditions are more easy to fulfil than it might appear. The whole operation consists in finding once for all the number of feet, fathoms, &c. which, multiplied by the difference of the logarithms, will reduce the abstract expression to one, giving absolute measures regulated by the ratio of the densities. Nothing is more simple provided we know this latter ratio. Let us suppose that at the pressure of 29·921 inches of mercury, and at the temperature of melting ice, the weights of air and mercury were as unity to 10477·9. The heights of the two columns being inversely as their densities, it is clear that we must ascend 1-100th of 10477·9, or 104·779, in order that the barometer may sink 1-100th of the same denomination as that in which our ascent is expressed, or nearly 8·7 feet, that it may sink 1-100th inch. Now the pressure being supposed equal to 29·921 inches, we shall find the number sought by dividing 104·779 by the difference of the tabular logarithms of the barometrical heights, 29·921 and 29·911.

The exact ratio of the densities being on the contrary supposed unknown, the operation will not be at all more difficult if we have measured geometrically and with great exactness a difference of elevation: for then, taking the barometer to the two extremities of the measured height, and dividing the difference of the logarithms by the difference of elevation, we shall equally obtain the number we seek; it will, however, correspond only to the particular temperature and pressure under the influence of which we have been operating. If, from hence, we wished to deduce the absolute ratio of the densities of mercury and air, we may arrive at it very easily by means of a formula, which is extremely simple, given in the "Astronomie Physique," of M. Biot.† My first memoir contains the application of the methods of proceeding which I have here alluded to.

* "Le Type de ces Mesures."

† Tom. i. p. 142.

This number, in fact, once determined either by observation or experiment, serves for all subsequent operations; by making the modifications which the difference of circumstances in each case requires: this is what we call the *constant coefficient* of the formula.

Thus, in order to measure the height of a mountain, the fundamental operation consists in observing the barometer both at the foot and the summit: taking out of the ordinary tables the logarithms corresponding to the barometric heights expressed in units of the same denomination and decimal parts of those units: subtracting the smaller from the greater logarithm, and multiplying the difference by the constant coefficient. The product will give the height required in measures of the same denomination as those which entered into the determination of the coefficient: and this height will be correct if we operate under the same circumstances which were supposed in determining the coefficient.

These circumstances are, as has been observed, a certain atmospheric pressure and a certain temperature, from whence results a certain ratio between the densities of air and mercury. The coefficient supposes them constant: they are in reality very variable; it must, therefore, undergo certain modifications analogous to the changes with which these circumstances may be affected.

In the formula of M. de Laplace, for example, the coefficient is determined for the level of the sea, the temperature of melting ice, and the latitude 45°. It is then only accurate for this single case, and the formula would be incomplete and inapplicable to other cases, if it did not comprise corrections suited to the variations of these first data.

The most important of these corrections relates to the variations of temperature; it is easy to conceive the principle of this, and to feel its necessity. Heat dilates air: it augments its volume, and diminishes its density. With equal weight, it occupies more space; with equal volume, it has less weight. If we suppose a stratum of air of 1000 feet in thickness intercepted between the levels of the base and summit of a mountain, this stratum will weigh less at a temperature of 10° centigrade than at zero. The difference of the heights of the barometer observed at the two stations will be less in the former than in the latter case; and if we apply the same coefficient to the two logarithmic differences, we shall have two very different measures of one and the same height; the mountain will seem to diminish in height in proportion as the temperature increases. Now the coefficient being calculated for the temperature of melting ice, we must, in consequence, increase or diminish its value, according as the temperature rises above, or sinks below, that point.

Experiment has taught us that the variation of air in volume is nearly 1-167th for a variation of 1° centigrade: supposing the

air to be in a state of absolute dryness; for the introduction of moisture sensibly changes this ratio. In fact aqueous vapour at the same temperature weighs less than air: and the stratum (which we took as an example) will again have its weight diminished without changing its temperature in proportion as it is mixed with a larger dose of moisture. Now as the atmosphere is never perfectly dry, we must add a correction for humidity to that which we employ for the temperature; and the hygrometer will serve to regulate this correction if we have for that purpose a sufficient number of observations which can be relied on; but considering that this correction will be in itself but very small, and that if we suppose the quantity of vapour constant, its variations will influence but little the exactness of the measurements, we may be satisfied to take the air at its usual state of a mean humidity, and to combine the two corrections by raising that for the temperature to 1-250th for each degree centigrade.

But in order to apply this correction, we must further consider what it is that we understand by the temperature of a column of air. We find that the heat decreases from the level of the sea to the highest regions we have been able to reach. A column of air is, therefore, more cold at its summit than at its base, and its mean temperature will be found between these extremes, at a distance regulated by the law which the decrease of temperature follows: if this decrease be uniform, that is, in arithmetical progression, we shall have the mean temperature by taking the mean of the thermometers at the two stations. This is the supposition most generally adopted. However some great philosophers think that the decrease is accelerated in proportion to the elevation. This may be true; but it is not less so, that it is extremely irregular; and whatever may be the general laws to which it is subject, these laws are altogether counteracted by the nature and form of the earth; by the reflection of the sun's rays, the variation of winds, and the action of ascending and descending currents; so that we may consider the hypothesis of an uniform decrease as a mean term from which we have, for the present, no reason to depart.

Another correction, of a more limited description, but not less important, is founded on the variation of gravity. It is well known that this force diminishes as we recede from the centre of the earth as the square of the distance. Now the earth is a spheroid flattened at the poles, and protuberant at the equator; the radii at the equator are longer than those at the pole; the polar, therefore, are nearer the centre than the equatorial regions; and gravity diminishes in proportion as we leave the former and approach the latter, for the double reason of the elongation of the radius, and the increase of the centrifugal force. It diminishes also, and even more rapidly when we rise above the mean level of the surface of the globe; and from these causes of the diminution of gravity, it follows as a necessary

consequence, that the established ratio between the densities of air and mercury must be altered. We easily conceive that two strata of air of equal weight, taken one at the equator, and the other at the pole, or the one at the level of the sea, and the other several thousand feet above it, will occupy more space in the latter situation than in the former, abstracting from all other causes of variation in the density. The same coefficient then cannot serve equally for these different cases, unless it be accompanied by a correction which increases or diminishes it in proportion to the variation of gravity. This correction naturally divides itself into two portions: for the diminution of gravity in the vertical line, it is founded directly on the general law, and extends equally to the weight of mercury and that of air. For the diminution upon the meridian, we find the measure of the correction in the length of the second's pendulum which requires to be shortened in proportion as it is less solicited by gravity. The two corrections have each a separate term allotted to them in the formula of M. de Laplace; and his coefficient being determined for lat. 45° at the level of the sea, the correction is plus in going towards the equator, and minus towards the pole; while in the vertical it remains always plus; only becoming subtractive when we descend below the level of the sea into the bowels of the earth, a case which never occurs, except in the bottom of deep mines.

One correction more completes the number of those which in the present state of our knowledge are essential to the accuracy of barometrical measurements; and this, though here treated of after the others, is nevertheless, in the order of the operations, the first to be effected.

It is evident that we shall but very imperfectly compare the heights at which two barometers are sustained, if we have not carefully observed the temperature of each. The point at which the mercury stands is determined not merely by the pressure of the atmosphere, but by the density also of the liquid which forms the counterpoise to it. Now heat dilates mercury, and diminishes its density. In that instrument then, of the two which is the warmest, the column of mercury rises in the tube to compensate by an augmentation of volume the portion of weight which it has lost. If we try the experiment of placing two barometers, of perfectly similar construction, and which agree perfectly together, one in a hot apartment, the other in the cold air without, but exactly on the same level, we shall see the same atmospheric pressure expressed by very different heights of the mercury. If we take them one to the foot, the other to the summit of a mountain, we shall readily see that before we can form a correct estimate of the difference of pressure, we must necessarily take into account the difference of temperature; the correction which this circumstance requires is of the most easy description. It results from very exact experiments that a

column of mercury expands $\frac{100}{5412}$ in passing from the temperature of melting ice to that of the ebullition of water, which is equivalent to $\frac{1}{5412}$ for each degree centigrade.

The outline which we have now given contains in an abridged form all that we at present know concerning barometrical measurements, their fundamental principles, and the auxiliary operations which they require. Until of late years regard was only paid to a part of the conditions of the problem; the others, although understood, and even pointed out by distinguished philosophers, remained without practical application. They have been all united for the first time in the excellent formula of M. de Laplace, a formula entirely founded on the general laws of the equilibrium of fluids, and which is not less remarkable for its exactness than for its generality. Geometers will find in all the works recently published on the subject the demonstrations and analytic developements of the propositions which I have done no more than enunciate; but it is in the "Mécanique Céleste," that minds familiarized with the most abstruse speculations of science will be gratified in finding the theory of the barometer connected with the immense series of physical laws, which together constitute the system of the world.

Method of Calculation.

The formula of M. de Laplace reduced to the most convenient form for calculation may be thus given:

Let z = the difference of elevation of the two stations.

h = the height of the barometer, T its temperature, and t that of the air at the lower station.

h', T$'$, t', the same at the upper station.

We have then the following equation:

$$z = \log. \left(\frac{h}{\mathrm{H}}\right) . 60158{\cdot}39 \text{ feet} . (1 + {\cdot}0028371 \text{ , cos. } 2\,\psi)$$

$$\left(1 + \frac{2\,(t + t')}{1000}\right) \left\{1 + \frac{\left(\log. \left(\frac{h}{\mathrm{H}}\right) + {\cdot}868589\right) \frac{z}{a}}{\log. \left(\frac{h}{\mathrm{H}}\right)}\right\}$$

In this equation $\mathrm{H} = h' + h' \left(\frac{\mathrm{T} - \mathrm{T}'}{5412}\right)$. ψ represents the latitude: a, the radius of the earth, = 20881129·44 feet: and we may put in the place of z, in the second member, its approximate value, namely, the second member itself without the last factor.

I have reason to believe that it is not easy to find another mode of proceeding, or to represent the algebraical quantities by a smaller apparatus of figures. Now though the calculation may neither be very intricate nor very long, it will still try the patience of those who have a great number of heights to calculate at one time; and barometrical operations are themselves of

so expeditious a nature, that quickness may be regarded as a suitable condition in the calculation.

I have determined, therefore, to make a slight sacrifice of rigorous exactness in order to afford philosophers the advantage of knowing the result of an observation in less time than would otherwise have been required. My mode of proceeding consists in regarding as constant the fourth factor of the above formula, by giving it the value which it would have at an elevation of about 9842 feet, at lat. 45°, and at a mean temperature of 15° centigrade. For this purpose it will only be necessary to replace the factor in question by an augmentation in the coefficient calculated according to the supposition just made. The formula will then become,

$$z = \log. \left(\frac{h}{H}\right) 60345{\cdot}4 \text{ feet} . (1 + {\cdot}0028371 . \cos. 2\psi)$$
$$\left(\frac{1000 + 2(t - t')}{1000}\right).$$

This formula without doubt is not rigorously exact; it exaggerates a little lesser heights, and diminishes a little those which exceed 9842 feet. We have only to inquire into the extent of this inaccuracy. For most elevations it will be much under 3 feet; and we must go to the equator, and ascend Chimborazo to find 8 feet difference between the results of the approximate and exact calculation; and 8 feet are, relatively to the height of Chimborazo what about half a foot is to most ordinary elevations; a quantity too small to be indicated by the instruments, and covered in the uncertainty of observation. I, therefore, see no reason for abandoning a mode of proceeding so convenient, and I have never employed any other to arrive at results whose exactness has been proved by the test of geometrical measurement; but what above all recommends it is, that M. de Laplace himself has not disdained adopting it in the third edition of his "Système du Monde," and that M. Biot has made it the basis of his barometrical tables, by deducing my coefficient 60345·4 from his own 60135, by a mode of analysis peculiar to himself.

When, however, we possess a formula of such an order as that which we owe to the author of the "Mécanique Céleste," we always regret the want of being able to represent in calculation the lesser quantities. M. Oltmanns has been unwilling to neglect those which I have, and he has improved my suggestion by taking into account the variations of $\frac{h}{H}$, but neglecting as I do the small products which refer to the variation of the latitude, and that of the temperature. His method is as follows: we have allowed that in the last factor of the formula, the value of z might be represented by the second member of the equation without this factor. In this expression, the constant coefficient being reduced into toises, and a mean value being given to the

other terms, it will be reduced to log. $\left(\frac{h}{\mathrm{H}}\right)$, 10000 toises very nearly, and the last factor of the formula will be:

$$1 + \frac{\left(\log. \frac{h}{\mathrm{H}} + \cdot 868589\right) \frac{z}{a}}{\log. \left(\frac{h}{\mathrm{H}}\right)} = 1 + \frac{(\log. \left(\frac{h}{\mathrm{H}}\right) + \cdot 868589)}{\log. \left(\frac{h}{\mathrm{H}}\right)}.$$

$$\left(\frac{\log. \left(\frac{h}{\mathrm{H}}\right) \; 10000}{a \text{ toises}}\right) = 1 + \frac{(\log. \left(\frac{h}{\mathrm{H}}\right) + \cdot 868589)}{\frac{a}{10000}}.$$

Under this form the last factor has only a single variable element; namely, log. $\left(\frac{h}{\mathrm{H}}\right)$. It becomes then very easy to calculate, and to reduce into an auxiliary table. This M. Oltmann's has done in the barometrical measurements of M. Humboldt, p. 71.

But it was possible to go still further, and I have attempted in my turn to improve upon the method of M. Oltmann's by the same means of which he has taught us the use.

In examining his supposition, I observe that it is only rigorously exact for the mean temperature of 15° at the equator, and 15·7° at lat. 45°. Elevations are often to be measured at a temperature very different from this, and I have thought it necessary to take into account in this factor the variations of $\frac{2\,(t\ +\ t')}{1000}$. It will suffice for this purpose to suppose the coefficient 60158 successively diminished and increased by quantities corresponding to different temperatures, and to transform the table of M. Oltmann's to one of double entry, having for its arguments $\left(\frac{h}{\mathrm{H}}\right)$, and $2\,(t\ +\ t')$; that is to say, the difference of the logarithms, and the double sum of the thermometers.

In this way there is nothing neglected in the correction for the diminution of gravity, except the part belonging to the latitude, or $1\ +\ \cdot 0028371\ .\ \cos.\, 2\,\psi$. But taking it from the mean latitude to the equator, it only affects the factor as if there were a variation of 0·7° in the temperature, which is altogether insensible, since a variation of 2·5° only affects it by a unit in the fifth decimal place of the logarithm of the correction.

Use of the Tables.

The whole operation of using the tables will be sufficiently obvious to those who have had the least practice in matters of this kind; but some explanation is necessary for the sake of those who are less versed in such operations.

The operation is simply this: we suppose the barometers to have been well compared together, and the thermometers divided

with the centigrade scale. It is indifferent by what scale the barometers are divided, so long as the two instruments have similar scales, and the units of the division are again subdivided into decimal parts. The observed readings off of the instruments are to be first written down. The logarithm of the height of the lower barometer is then to be taken from the ordinary tables. It is at this point of the operation that it is convenient to proceed to the correction for the temperature of the instrument. The table (No. 1) gives the logarithms of these corrections for degrees and tenths of the centigrade thermometer. The difference is positive when the lower barometer is the warmer, which is the most common case; and negative when the upper. This table gives the logarithm to be added to that of the lower barometer to reduce the instruments to the same temperature. We then proceed to take the logarithm of the upper barometer, and the difference of these is the number proportional to the difference of elevation, which we have now to reduce into absolute measures.

This conversion is effected by multiplying this difference by the coefficient, accompanied by the corrections belonging to the mean temperature of the column of air, the latitude of the place, and the diminution of gravity in the vertical direction.

We begin by taking the logarithm of the difference of logarithms; to this is then added,

1. The logarithm of the constant coefficient for lat. 45° reduced to that sort of measure in which we wish to have the result. By keeping this primitive coefficient separate from the modifications introduced by the latitude, we have a facility of calculating heights in any measures, or of altering the coefficient if it should be found necessary.

2. The correction for the latitude. This need only be calculated for intervals of 1°: a greater degree of exactness would be superfluous. (Table 2.)

3. The correction for the vertical diminution of gravity. (Table 3.)

(For the details of these operations, see the remarks and examples accompanying the tables.)

In the table of this correction, the first two decimals of the difference of logarithms in the vertical, and intervals of 10° of the thermometer in the horizontal column, are sufficient. The mean differences belonging to each column afford the means of calculating to the third decimal, or to intermediate degrees of the thermometer if we wish. This is generally unnecessary, but these differences have another use, which we shall see as we proceed.

The correction for the diminution of gravity supposes the lower barometer to be at the level of the sea. So long as the inferior station is much elevated, the correction regulated from this point of departure becomes insufficient to apply to the other

station; and geometrical exactness requires that we should augment the correction in proportion to the height of the station; but it must be allowed that cases where this is necessary are very rare, and its effect on the exactness of measurements is very inconsiderable. If the lower station be much elevated, we shall rarely have a great height to measure above it: if but little, we shall have only a very small correction to make; so that the small quantities which it introduces into the calculation will generally be covered by the uncertainty from which no observation is exempt.

However, the learned coadjutor of M. de Humboldt has been unwilling to neglect this correction; and we find in the hypsometrical tables which he has just published, a small table of quantities to be added to the measured heights according to the absolute elevation at which the lower barometer is placed; an elevation sufficiently indicated by that of the column of mercury. I have borrowed from him this table, merely transforming it into logarithms to agree with the system of calculation which I have adopted (Table 6). The divisions of the barometer in the first column are sufficiently near for the degree of accuracy required: if greater exactness be desired, it may be sufficiently attained by means of the column of differences.

It only remains to take into account the mean temperature of the column of air. The mode of effecting this correction according to the formula is extremely simple. The double sum of the thermometers will be positive or negative as it is above or below zero: the operation is obvious from the following example:

$$\text{Let } 2\,(t + t') = +\,40;\ \text{we have } \frac{1040}{1000} = 1{\cdot}040$$

$$\text{If it be } = -\,40,\ \text{it becomes } \frac{960}{1000} = 0{\cdot}960$$

For the logarithms of these numbers, the observer will at once refer to the common tables. To have given a table of them would have been merely a superfluous transcription.

The sum of the five logarithms thus obtained, and which it is convenient to write in a column for addition, is the logarithm of the elevation required, expressed in measures of the same kind as those to which the constant coefficient has been reduced.

It is obvious then that this slight operation is much easier to perform than to explain: it is reduced to transcribing numbers previously prepared. No auxiliary calculations—no taking of proportional parts—no interpolations are requisite. My tables are constructed in such a form as to furnish at once all the fractions which are worth taking into account. They cannot possess this advantage without some want of brevity; but it is a matter of great indifference for the tables to be somewhat long, while it is by no means so that the calculation should be short, clear, and easy to verify in all its parts. In the method which I recommend,

the observations being prepared, as in all possible methods they must be, there are literally only two operations to perform; a subtraction and an addition; for surely no one will consider worthy the name of an operation the slight trouble of searching in the tables for numbers already calculated.

It remains for me to point out the resources which these tables present in those cases, assuredly very rare, where an observation may be made in circumstances not included within the limits of the tables.

We should not be surprised, for instance, if the prodigious height to which M. Gay-Lussac was elevated in his aërial voyage, should not be comprehended in the table for the vertical diminution of gravity; but it is easy to provide for such a case by means of the differences placed at the bottom of the column. I obtain the logarithm belonging to the logarithmic difference 0·36 in the column + 40, by adding ten times the mean difference 129·7, or 1297, to the logarithm 0·0014658, which belongs to the difference 0·26 in the table.

In the same way, when we measure very small heights, the double sum of the thermometers may sometimes exceed the limits of the table. Thus, for example, if the double sum be 107, and the logarithmic difference 0·005, I extend the series corresponding to that difference to the column of 110° which is wanting in the table, by adding the mean difference 108·9 to the logarithm 0·0012003 in the column of 100°. This difference must be subtracted if we wanted a number in the column of — 20; but this excess of precision in small heights will readily appear useless. We may confine ourselves to the logarithm in the nearest column to that which is wanting.

The observation of Gay-Lussac makes the first table for the temperature of the instrument equally insufficient. But with what tables, if we except the logarithmic, will not this, in such cases, happen. The difference of the thermometers was 40·3°; the table only extends to 30°. We may supply the deficiency without sensible error by adding to the logarithm answering to 30° that which answers to 10·3°. But it would be both as expeditious and more convenient to correct directly the height of the colder barometer by means of table No. 4, which gives the augmentation corresponding to a difference of temperature from 1° to 10°: in many cases this may probably be the preferable method.

It has also another advantage. M. Daubuisson has recently proposed to add to the correction for the temperature of mercury a second correction for the dilatation of brass, when we employ barometers whose mounting is made of this metal. This correction is very small, for the dilatation of brass is only about the tenth part of that of mercury; namely, nearly $\frac{1}{54000}$ for 1°. But for the sake of exactness nothing should be neglected, and the

proposition of M. Daubuisson deserves to be received. Table, No. 4, renders this correction extremely easy: the whole operation is reduced to diminishing by a tenth the number which the table gives for mercury.

The correction for the dilatation of brass always diminishes the correction for the dilatation of mercury, and even in the case where we reduce the temperature of the warmer to that of the colder barometer. In fact, the pyrometric variation of the scale proceeds in an opposite direction to that of mercury. If heat lengthen the scale, the column of mercury is measured by a less number of divisions: if cold contract it, by a greater. In the first case, the quantity to be subtracted from the height of the mercury is diminished by the quantity to be added to the length of the scale. In the second case, the quantity to be added to the height of the mercury is diminished by that which is to be subtracted from the length of the scale. This rule must not be forgotten, if we adopt the plan of reducing all observations to a constant temperature, as, for example, to 12·5°.

In respect to using these tables, it may be observed, that it is altogether indifferent by what scale the barometer is divided, provided its lesser divisions be always expressed in decimal parts of the integers.

It is of consequence to the accuracy of barometrical measurements, to have barometers in which the mercury stands at its real and absolute elevation. In instruments of the cistern construction, this is never the case. The column is depressed owing to the force of capillarity, which is the more considerable in proportion as the diameter of the tube is less: it exceeds ·039 inch in tubes whose interior diameter is from ·19 to ·20 inch. M. de Laplace has calculated a table of these depressions which is here given. In order to use it, we must measure accurately the interior diameter of the tubes we employ, and add to the height of the mercury, the quantity in the table, answering to the number nearest the given diameter.

The method of calculation just explained, although very expeditious, may be, perhaps, somewhat less so than methods founded on tables specially adapted for giving the result at once, and particularly if such tables are carried to such an extent as to dispense with all interpolation; but it will always retain the advantage of a greater generality, and it appears to me to recommend itself not only by the exactness and facility with which it is performed, but above all by the convenience which it possesses of being equally suited to calculators of all countries; and of admitting any alteration which it may be judged necessary to make in the constant coefficient.

These considerations, however, diminish nothing of the merit of many very ingenious methods which have been substituted for the logarithmic; but of these the most remarkable for brevity have necessarily the fault of leaving to the charge of the

observer the lesser calculations incident to them, which I have made it my business to supersede; and all these methods have the inconvenience of requiring the exclusive use of certain measures, or obliging the calculator to go through reductions which greatly lengthen the operations, and multiply the causes of error. The logarithmic method is perfectly independent of different systems of measures. The observer must, it is true, burden himself with a set of logarithmic tables, but it is very easy to separate that part of them which contains the series of numbers wanted, and this joined with the tables here given can never be considered a great incumbrance to a traveller who takes the trouble of carrying a barometer to the summits of mountains.

Isolated Observations.

With respect to the decrease of temperature as we ascend in the atmosphere, nothing seems certain. Near the surface of the earth, the decrease of temperature is commonly very slow; sometimes however very rapid. The rate of decrease is commonly accelerated at a certain height, and the maximum of acceleration is found in a stratum of air whose absolute elevation seems to vary with the climate. Near the equator, M. de Humboldt has found it between 8,200 and 11,480 feet. In the Pyrenees, I have found it between 6000 and 9000 feet: above this, it proceeds again more slowly; and this general disposition of circumstances is again modified and disturbed in a thousand ways by the influence of seasons, of situations, of winds, of ascending and descending currents, of the sun, clouds, rain, &c.; so that when we form an opinion of the rate of decrease from observations at two or three points of a measured scale of elevation, we commonly find the law defective for all intermediate points. The supposition of an uniform decrease adopted in all our formulæ is a mean value which accords with the greater number of cases, and holds, as it were, an even balance between a multitude of opposite results. This supposition is in all respects sufficient to answer the purpose for which it is chiefly wanted, the measurement of mountains; about which the air subjected to the reaction of the earth, exhibits effects very different from those produced in its state of absolute independence; and as far as observation has yet gone, this mode of proceeding is justified by the accuracy of our measurements, as well as recommended by its simplicity.

The extreme irregularity which affects the decrease of temperature in the stratum of air next the earth, is one of those obstacles which must always present themselves in any attempt to determine elevations exactly, without corresponding and simultaneous observations of the barometer and thermometer. It may not be useless to make a few remarks on observations of this kind, as more confidence has been reposed in them by some philosophers than they appear to me to deserve.

I have collected a number of observations running through a scale of nearly 21,000 feet of elevation, and at temperatures varying from $-1°$ to $+28°$. Now whatever law of decrease we may have deduced from theory, from the abstract constitution of the atmosphere, &c. it will be impossible to make it agree with these results. Slow and rapid diminutions of temperature accompany indifferently both great and small elevations; we find them taking place indiscriminately in the higher, middle, and lower regions of the atmosphere; and at all degrees of heat and cold. Yet these are good observations, and have furnished in general very exact measurements, and many of them confirmed by geometrical determination. And they are moreover the same sort of observations which we commonly make, and for which our barometric formulæ are constructed. The measurements are found accurate, because the thermometer consulted at the lower station has eliminated an unknown quantity to which no theoretical considerations could have assigned a determinate value. A corresponding observation at the base of the column is a fixed point of departure; the extremes of the temperature being once known, correct the calculation; and although the decrease of temperature generally undergoes, in the same column of air, irregularities occasioned by a multitude of accidental causes, which by turns retard and accelerate it, and is sometimes inverted; yet an arithmetical mean taken between the extreme temperatures so well covers these irregularities, that the exactness of the measurements is not at all affected.

It may sometimes happen that we have not corresponding observations; and when we carry a barometer, we are desirous of deducing at once from it nearly the absolute elevation of the place. The expedient hitherto most commonly adopted has been to compare the observed height of the barometer with its mean height at the level of the sea; but in this method there is always an inherent fault: we compare an insulated observation with the mean of a great number of observations. The comparison will only be just in the single case when the barometer happens accidentally to be precisely at its mean height; in general, it may be considerably above or below it; there is then only one chance in favour of the accuracy of the measurement; that is, by allowing for the diminution of temperature according to the approximate elevation, and the error in this correction happening to compensate the former: if, however, the two errors instead of compensating each other, accumulate, the apparent amelioration of the calculation will have no other effect than to increase the inaccuracy. We may, however, proceed thus: Suppose the mean height of the barometer at the level of the sea to be 30·03437 inches at a temperature of 12·5°, a supposition adopted by most philosophers from the observations of Sir G. Shuckburgh. I commence by reducing my observation to the

same temperature by means of Table 4; I then take the difference of the logarithms: this gives a basis for finding the probable temperature at the lower station according to the most ordinary law of decrease. For this purpose it will suffice to reduce the difference to these decimals, and multiply it by the constant number 122. The product expresses in degrees and decimals, the quantity by which we must augment the temperature indicated by the higher thermometer to get approximately that at which the lower ought to stand. By this means the correction for the temperature of the air is applied.

The factor 122 expresses the mean rate of decrease as deduced from the collection of observations before-mentioned, and agrees with the law of decrease adopted by some philosophers from theory; but though this may inspire some confidence in the observations upon which the value of that factor depends, yet it inspires none for this mode of measuring heights. I find that this method does not sufficiently correct the deviations of the simple method; and moreover the errors instead of compensating each other, are almost always on the same side, and tend generally to diminish the real height. This tendency is remarkable; it has little to do with the decrease of temperature assigned; for we perceive it equally in the uncorrected method. It rather concurs with other circumstances to make me suspect what has been most generally considered the mean height of the barometer at the level of the sea. If I can sufficiently confide in my own observations, this mean is taken *too low*. If it be raised to 30·089 inches, for the hour of noon, and temperature 12·5°, there will be rather more equality between the chances of error in the different modes of calculation in which this mean is employed.

But whatever may be the corrections which we may apply to the approximate methods which have caused this digression, they will be always very faulty methods. We shall never have recourse to them but with distrust, and only in order to estimate within about 30 feet, the elevation of a place when we have no means of obtaining a more precise determination. The barometer after all does no more towards the measurement of heights without corresponding observations than the repeating circle without an exact determination of distances.

Mode of conducting the Observations.

To understand the theory of barometrical measurements is by no means difficult: it is even still easier to learn to *calculate* the observations well, but the most difficult thing is to *perform* them well. Very skilful persons have often given extremely deficient observations, as well from want of good instruments, as of good methods; and always from having considered as more easy than it really is, what appears to be a very simple physical experi-

ment; which is, however, in itself of an extremely delicate nature, and which frequently does not answer in the way in which we put the question; for this reason, that it is the nature of all experience not to give a satisfactory answer, except to questions very well instituted.

However, we are commonly in haste to draw conclusions; and men had not even thought of improving the instruments, and much less the methods of observing, when already the lesser as well as the greater questions of meteorology, had been investigated, if not considered as resolved, on the sole ground of observations, either insufficient, or suspicious, or which did not really say a word of what they were made to say.

(*To be continued.*)

Article IV.

Register of the Rain at Bombay in the East Indies, measured with Howard's Pluviometer daily at 7 a. m. By Benjamin Noton, Esq. of Bombay.

1817.	Inch.	1817.	Inch.	1817.	Inch.	1817.	Inch.	1817.	Inch.
June 1	—	July 1	0·09	Aug. 1	0·30	Sept. 1	0·04	Oct. 1	0·19
2	0·32	2	0·49	2	0·06	2	—		
3	—	3	0·05	3	0·24	3	0·09		
4	1·23	4	0·18	4	0·52	4	0·18		
5	6·46	5 }	0·27	5	0·08	5	0·07		
6	—	6 }		6	0·06	6	0·02		
7	0·03	7	0·15	7	—	7	1·33		
8	0·11	8	2·09	8	—	8	0·33		
9	0·80	9	0·55	9	—	9	2·68		
10	1·20	10	2·06	10	—	10	0·32		
11	1·57	11	0·17	11	—	11	0·87		
12	0·20	12	3·80	12	—	12	1·50		
13	2·30	13	0·99	13	—	13	4·08		
14	1·03	14	1·34	14	—	14	0·91		
15	—	15	2·57	15	—	15	1·25		
16	0·16	16	3·00	16	0·07	16	2·40		
17	1·38	17	0·40	17	—	17	0·42		
18	5·12	18	0·35	18	—	18	0·40		
19	1·85	19	1·91	19	—	19	—		
20	0·54	20	0·20	20	0·19	20	1·09		
21	—	21	0·25	21	0·10	21	1·75		
22	1·38	22	0·08	22	0·06	22	1·58		
23	9·03	23	0·35	23	—	23	1·25		
24	7·23	24	0·43	24	0·14	24	0·05		
25	0·15	25	0·54	25	—	25	—		
26	0·73	26	0·61	26	2·39	26	—		
27	—	27	—	27	2·66	27	0·21		
28	0·32	28	0·42	28	2·06	28	0·55		
29	1·43	29	0·05	29	0·13	29	—		
30	1·15	30	0·13	30	0·10	30	1·51		
		31	0·15	31	0·18				
	45·72		23·67		9·34		24·87		

1818.	Inch.	1818.	Inch.	1818.	Inch.	1818.	Inch.	1818.	Inch.
June 10	0·20	July 1	0·53	Aug. 1	0·24	Sept. 1	0·69	Oct. 1	2·07
11, 12	2·30	2	2·46	2	0·08	2	2·45		
		3	1·13	3	0·09	3	0·26		
13	2·10	4	0·48	4	4·47	4	0·41		
14	0·70	5	0·56	5	0·63	5	0·18		
15	1·50	6	0·31	6	3·20	6	0·61		
16	2·95	7	—	7	1·18	7	0·12		
17	2·65	8	0·11	8	2·05	8	0·18		
18	6·87	9	0·11	9	4·29	9	0·32		
19	0·26	10	0·12	10	1·68	10	0·64		
20	—	11	0·06	11	1·65	11	0·21		
21	1·34	12	0·24	12	0·30	12	0·07		
22	—	13	3·62	13	0·70	13	0·01		
23	0·02	14	1·43	14	0·35	14	—		
24	—	15	0·18	15	0·19	15	—		
25	—	16	0·16	16	0·69	16	0·05		
26	—	17	0·51	17	0·04	17	0·29		
27	0·13	18	1·86	18	0·12	18	0·10		
28	0·63	19	0·50	19	0·46	19	—		
29	0·48	20	0·63	20	0·22	20	0·43		
30	0·41	21	0·09	21	0·72	21	0·03		
		22	0·10	22	2·58	22	—		
	22·54	23	—	23	0·27	23	—		
		24	0·11	24	0·32	24	0·34		
		25	—	25	0·18	25	0·89		
		26	0·01	26	0·56	26	1·86		
		27	0·14	27	0·04	27	—		
		28	0·33	28	0·10	28	0·06		
		29	0·05	29	0·35	29	0·13		
		30	1·43	30	0·18	30	0·06		
		31	0·43	31	0·52				
			17·69		28·45		10·39		

1819.	Inch.	1819.	Inch.	1819.	Inch.	1819.	Inch.	1819.	Inch.
June 1	—	July 1	0·09	Aug. 1	1·02	Sept. 1	—	Oct. 1	—
2	—	2	—	2	0·14	2	—	2	0·14
3	0·29	3	0·18	3	0·04	3	0·91		
4	0·11	4	0·30	4	2·53	4	0·06		
5	0·55	5	0·12	5	0·07	5	0·31		
6	1·35	6	0·20	6	1·46	6	0·20		
7	2·67	7	1·66	7	—	7	0·30		
8	0·76	8	0·97	8	1·43	8	0·89		
9	0·95	9	1·03	9	1·85	9	0·01		
10	0·50	10	0·23	10	0·17	10	0·05		
11	—	11	0·52	11	1·20	11	0·19		
12	—	12	0·29	12	2·68	12	0·13		
13	0·43	13	0·75	13	0·24	13	0·08		
14	0·38	14	0·98	14	0·38	14	0·19		
15	0·72	15	2·40	15	0·53	15	0·43		
16	0·01	16	3·65	16	0·82	16	0·10		
17	0·26	17	1·53	17	0·31	17	0·12		
18	0·16	18	0·79	18	0·72	18	0·05		
19	0·03	19	0·39	19	0·87	19	0·08		
20	0·16	20	0·21	20	1·44	20	0·08		
21	1·78	21	0 33	21	0·18	21	1·00		
22	1·50	22	0·78	22	0·48	22	1·20		
23	0·31	23	2·21	23	0·32	23	0·07		
24	—	24	6·91	24	0·43	24	0·84		
25	—	25	2·40	25	0·24	25, 26, 27	1·34		
26	0·78	26	0·13	26	0·01				
27	0·16	27	—	27	—				
28	1·95	28	0·14	28	0·50	28	0·34		
29	—	29	2·25	29	0·12	29	0·94		
30	0·14	30	0·10	30	0·01	30	0·20		
		31	0·12	31	0·05				
	15·95		30·66		20·24		10·11		

1820.	Inch.	1820.	Inch.	1820.	Inch.	1820.	Inch.	1820.	Inch.
May 11	0·24	June 11	0·16	July 1	0·13	Aug. 1	—	Sept. 1	—
		12	—	2	0·16	2	—	2	0·01
		13	0·24	3	0·31	3	—	3	0·08
		14	0·39	4	3·94	4	0·25	4	—
		15	1·62	5	0·63	5	0·10	5	0·03
		16	0·68	6	3·35	6	1·46	6	—
		17	0·10	7	0·36	7	0·39	7	0·06
		18	0·38	8	1·53	8	2·47	8	0·28
		19	1·46	9	8·21	9	0·81	9	0·06
		20	3·83	10	2·14	10	1·31	10	0·37
		21	2·81	11	0·35	11	0·25	11	—
		22	3·67	12	0·10	12	0·23	12	—
		23	1·30	13	0·17	13	0·47	13	—
		24	0·60	14	0·06	14	0·44	14	0·21
		25	0·16	15	0·63	15	—	15	0·76
		26	0·02	16	0·41	16	5·83	16	0·12
		27	0·11	17	0·22	17	—	17	2·86
		28	0·13	18	0·34	18	0·46	18	1·71
		29	0·40	19	0·07	19	0·74	19	1·98
		30	0·52	20	—	20	1·18	20	0·35
			18·58	21	0·34	21	0·79	21	1·31
				22	0·73	22	0·57	22	0·37
				23	0·66	23	1·06	23	0·04
				24	2·44	24	0·11	24	—
				25	0·34	25	0·37	25	0·06
				26	0·15	26	0·10	26	—
				27	0·33	27	0·01	27	—
				28	0·11	28	—	28	—
				29	0·09	29	—	29	—
				30	0·07	30	—	30	—
				31	—	31	0·03		
					28·37		19·49		10·66

1821.	Inch.	1821.	Inch.	1821.	Inch.	1821.	Inch.	1821.	Inch.
June 17	0·26	July 1	0·21	Aug. 1	0·45	Sept. 1	0·13	Oct. 3	0·40
18	0 ·2	2	0·21	2	0·26	2	—		
19	—	3	0·29	3	0·07	3	—		
20	0·97	4	0·06	4	0·50	4	0·06		
21	0·32	5	0·97	5	4·18	5	—		
22	3·56	6	0·21	6	2·08	6	0·14		
23	0·31	7	0·08	7	3.68	7	0·05		
24	0·33	8	1·19	8	1·52	8	—		
25	0·26	9	0·92	9	3·80	9	—		
26	3·27	10	0·05	10	7·96	10	—		
27	0·94	11	0·93	11	0·49	11	1·07		
28	1·85	12	0·15	12	1.00	12	0·57		
29	0·89	13	0·58	13	0·26	13	4·68		
30	2·02	14	0·08	14	0·16	14	0·26		
		15	0·48	15	0·35	15	1·15		
	15·18	16	2·23	16	0·16	16	0·55		
		17	1·02	17	0·13	17	0·62		
		18	0·41	18	0·18	18	0·78		
		19	0·64	19	—	19	0·84	1822.	
		20	3·50	20	0·05	20	0·46	May 28	0·19
		21	2·14	21	0·37	21	1·48	29	0·09
		22	0·61	22	0·05	22	0·13	30	0·15
		23	1·03	23	0·08	23	0·30	31	—
		24	0·35	24	0·03	24	0·72		
		25	0·06	25	—	25	2·00		
		26	0·06	26	0·04	26	0·02		
		27	0 60	27	—	27	—		
		28	0·17	28	0·12	28	—		
		29	0·21	29	0·16	29	2·28		
		30	1·00	30	0·29	30	—		
		31	0·16	31	0·10				
			20·60		28·52		18·29		

1822.	Inch.	1822.	Inch.	1822.	Inch.	1822.	Inch.	1822.	Inch.
June 1	0·30	July 1	0·63	Aug. 1	0·01	Sept. 1	0·17	Oct. 1	—
2	0·20	2	0·05	2	0·68	2	0·16	2	0·09
3	—	3	1·90	3	4·04	3	6·26	3	0·10
4	—	4	4·06	4	0·02	4	5·15	4	—
5	0·25	5	1·43	5	0·04	5	5·55	5	—
6	0·75	6	3·20	6	—	6	2·65	6	—
7	0·95	7	1·30	7	0·04	7	0·59	7	—
8	0·27	8	2·23	8	2·15	8	0·36	8	0·34
9	—	9	0·62	9	1·66	9	0·08	9	0·15
10	0·01	10	0·10	10	2·05	10	—	10	0·14
11	1·32	11	0·01	11	0·25	11	—		
12	0·32	12	0·31	12	0·96	12	—		0·82
13	0·44	13	0·01	13	1·12	13	0·26		
14	0·52	14	0·06	14	1·40	14	0·21		
15	0·18	15	0·93	15	1·59	15	0·09		
16	0·12	16	0·48	16	0·11	16	0·23		
17	0·18	17	1·52	17	0·04	17	0·06		
18	2·40	18	2·31	18	0·07	18	—		
19	4·50	19	2·66	19	—	19	—		
20	4·11	20	0·60	20	—	20	—		
21	1·10	21	0·42	21	—	21	0·16		
22	5·33	22	—	22	—	22	—		
23	2·04	23	0·06	23	0·55	23	—		
24	0·56	24	0·40	24	0·14	24	0·03		
25	0·20	25	0·90	25	0·96	25	—		
26	0·23	26	0·06	26	0·05	26	0·10		
27	0·29	27	—	27	0·89	27	0·06		
28	0·40	28	0·18	28	3·70	28	—		
29	0·74	29	0·02	29	3·45	29	—		
30	1·07	30	—	30	7·64	30	—		
		31	0·14	31	0·22				
	28·78		26·59		33·83		22·16		

1817.		1820.	
June	45·72	May	0·24
July.	23·67	June.	18·58
August.	9·34	July	28·37
September	24·87	August	19·49
October	0·19	September	10·66
	103·79		77·34
1818.		1821.	
June.	22·54	June.	15·18
July	17·69	July	20·60
August.	28·45	August	28·52
September	10·39	September	18·29
October.	2·07	October.	0·40
	81·14		82·99
1819.		1822.	
June	15·95	May.	0·43
July	30·66	June	28·78
August.	20·24	July	26·59
September.	10·11	August.	33·83
October	0·14	September	22·16
		October	0·82
	77·10		112·61

Statement showing the Fall of Rain at Bombay in the last Six Years, measured with Howard's Pluviometer.

Year.	June.	July.	August.	Septemb.	October.	Total.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1817	45·72	23·67	9·34	24·87	0·19	103·79
1818	22·54	17·69	28·45	10·39	2·07	81·14
1819	15·95	30·66	20·24	10·11	0·14	77·10
1820	18·82	28·37	19·49	10·66	—	77·34
1821	15·18	20·60	28·52	18·29	0·40	82·99
1822	29·21	26·59	33·83	22·16	0·82	112·61

Article V.

An Account of some Experiments with the Prism. By S. L. Kent, Esq. MGS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Carpenter's Hall, July* 16, 1823.

In offering to you the following details of a few simple experiments with the prism, I am not impelled by the belief that they may prove of any practical utility, or serve to throw any new light on the doctrines relating to colours, to which I have given little or no attention myself; they will, however, evince that this instrument affords the means of passing a few hours very agreeably. Leaving to others more conversant with such pursuits to look into them for any instruction they may possibly afford, I cannot however refrain from noticing the following experiment made by Dr. Wollaston, Phil. Trans. 1802, vol. 92, p. 2:—

"If a beam of daylight be admitted into a dark room by a crevice 1-20th of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint glass, free from veins, held near the eye, the beam is seen to be separated into the four following colours only: red, yellowish-green, blue, and violet."

My seventh experiment, however, tends to the reduction of the prismatic colours into three primary ones, wanting the blue one observed by Dr. Wollaston. I beg to add that I am not aware that any one of these seven experiments have hitherto been made, or described by any other person, and am, Sir,

Your humble servant,

S. L. Kent.

P. S. I should add that the prism used in these experiments is five inches long, and the side planes one inch broad; the lens

is six inches in diameter, having a focus of two feet three inches; and I may mention, that I found it requisite that the diameter of the lens should exceed the length of the prism in order to insure a good spectrum.

Exper. 1.—I threw the colours of the prism on a screen, eleven feet distant, and having placed the lens between them, and only two inches from the prism, I found the prismatic colours magnified, and in the same order, to the dimension two feet six inches in width, and one foot three inches in depth. In this case the sun's rays were admitted through a Venetian blind; but when admitted through a hole in a shutter of five inches by four, the dimension was only two feet by nine inches.

Exper. 2.—Having placed the lens at the distance of two feet six inches from the prism, the figure of the prism was clearly defined, but without exhibiting any prismatic colours whatever on the screen.

Exper. 3.—I placed the lens three feet from the prism, which produced only the figure of the prism having the violet ray at the bottom, and the yellow above.

Exper. 4.—When the lens was five feet from the prism, the figure of it was distinctly seen with the prismatic colours reversed.

Exper. 5.—I placed the lens behind the prism, and threw the sun's rays on it at its focal distance two feet three inches, when the prismatic colours were increased, both in brilliancy and magnitude, considerably more than in Exper. 1.

Exper. 6.—I put the lens within the focal distance of the screen, when a small figure of the prism was seen very bright, but without any prismatic colour.

Exper. 7.—Having placed the lens as in Exper. 2, when no prismatic rays were produced, but a perfect spectrum of the prism in a strong white light; I then placed another prism in the focus of the lens, and to my surprise it produced three colours only, viz. yellow of a greenish tint, red, and deep violet. Wishing to ascertain if those three colours were neutral, I tried them with a third prism, and found not the slightest alteration; and having placed a card so as to receive them, I found, on giving it a whirling motion, that the colours were entirely lost.

Article VI.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 43.)

The crystallographical characters of natural and artificial productions appear to have received less general attention than the other branches of science connected with mineralogy. I have already alluded to the inadequate descriptions of crystalline forms contained in Dr. Henry's excellent work on Chemistry; and I may refer to another recent and valuable publication which happens to lie before me, Dr. Ure's Dictionary, for abundant evidence of the neglect which the crystallographical character has experienced among chemists of the first rank.

Crystalline forms which are incompatible with each other are frequently quoted in these works as belonging to the same substance; and sometimes those forms are described in terms to which no very definite meaning can be attached; as where *Andalusite* is said, in Dr. Ure's work, to crystallize occasionally in *rectangular four-sided prisms verging on rhomboids.*

The crystalline form of morphia is given in Dr. Henry's work, on the authority of three different chemists, as a *rectangular prism with a rhomboidal base;* as a *regular parallelopiped with oblique faces;* and as a *four-sided rectangular prism;* and Dr. Ure quotes the form given by Choulant, as a *double four-sided pyramid with square or rectangular bases.* The first of these forms is impossible, unless we suppose the base *oblique* to the axis of the prism, and then it is incompatible with the third and fourth. The second is not very intelligibly described. The last two are not incompatible with that which is given below.

If we inquire into the causes which have occasioned this neglect of a science, not really difficult in itself, we shall perhaps find that it is owing chiefly to the very profound manner in which it has been treated by the late Abbé Haüy, in whose hands the subject first assumed a strictly scientific form. His complicated analytical operations were probably repulsive to most readers, and so much so, that even in France there are scarcely, as I have been very recently informed by one of his friends, a dozen persons who have followed him in his researches.

Another cause of the little acquaintance which appears generally to exist with even the forms of crystals, may, perhaps, be traced to the nomenclature which the late Abbé established to designate them; by this they were presented to the reader as

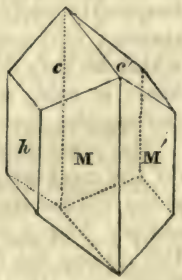
independent rather than as related forms, and the mind was thus led away from the consideration of their relations to each other, rather than assisted in comprehending them.

It is probable that the study of crystals will be much assisted by a general series of forms, serving as a type, with which all the crystals of different substances might be readily compared. This series I have attempted to supply in the volume already alluded to, which contains tables of all the modifications of which the simple crystalline forms are susceptible.

The letters placed on the figures which accompany these remarks correspond with those used in the tables here referred to; and by means of these, the reader may trace the relations of all the planes on these figures, to the simple primary form from which they are supposed theoretically to be derived. I have, therefore, omitted, in most instances, to give a figure of the primary form of the substances described.

Morphia.

These crystals are very minute, and have only one cleavage that I can perceive, parallel to the plane *h*. The primary form is a *right rhombic prism*, only the lateral planes of which appear on the crystals. For these I am indebted to Mr. R. Howard, of Stratford.



M on M′	127° 20′
M on *h*	116 20
h on *c*	132 20
c on *c′*	95 20

Tartaric Acid.

The crystals from which this form has been determined, were also given to me by Mr. R. Howard. I have not succeeded in cleaving them, but the primary form is *an oblique rhombic prism*. Fig. 1 exhibits the crystal as usually modified, with the planes symmetrically placed. Fig. 2 exhibits the same modified form, with the planes irregularly disposed as they appear in most of the crystals, the corresponding planes in both being marked with the same letters. This affords another instance of irregularity, which renders it not easy immediately to perceive the relations of the several planes to each other.

Fig. 1.

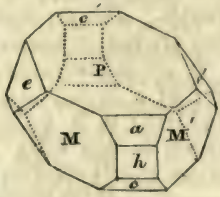
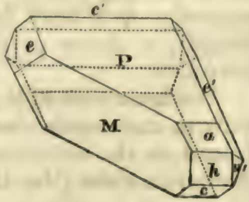


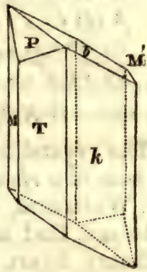
Fig. 2.



P on M, or M′	97°	10′
M on M′.	88	30
P on *e*, or *e*′	128	15
P on *a*.	134	50
P on *h*	100	47
P on *c*′.	122	45

Gallic Acid.

These crystals, which were prepared by Mr. R. Phillips, and are very minute, have one distinct cleavage parallel to the plane P, and apparently another parallel to M. The primary form is a *doubly oblique prism*, and the measurements are as follows:



P on M.	95°	00′
P on T.	125	20
M on T.	84	00
T on *k*.	160	00
k on M′.	116	00
P on *b* about.	116	00
b on M′ about........	150	00

(*b* is a very dull plane.)

Oxalic Acid.

The primary form is *an oblique rhombic prism.* There are distinct cleavages parallel to the planes M and M′, but I have not observed any other. The crystals are usually attached by one of the lateral ends of the figure, in consequence of which the planes P, *a*, and *c*, appear like lateral planes of a prism, and M, M″, as its dihedral termination.

Fig. 1.

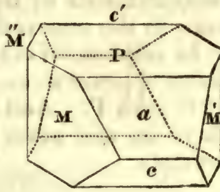
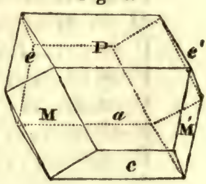


Fig. 1 exhibits the common form of the crystals; and fig. 2 a modified form which sometimes occurs, and not unfrequently with only one of the planes *e* apparent at the lateral extremity, the other not being visible.

Fig. 2.



P on M, or M′	98°	30′
M on M′.	63	5
P on *a*.	129	20
P on *c*′	103	15
P on *e*, or *e*′.	107	00

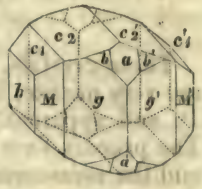
Citric Acid.

Cleaves readily parallel to the planes M, M′, and *h*, of the annexed figure, but I can observe no cleavage in any other direction.

From the character of the secondary planes, the primary form is *a right rhombic prism*, and the measurements taken chiefly on a crystal I received from M. Teschemacher, are nearly those

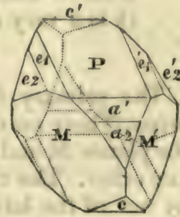
which follow. The crystals, however, so speedily lose their brilliant surfaces when exposed to the air, or even when inclosed in a bottle, that the measured angles of the secondary faces are less to be relied upon than those afforded by the cleavage planes.

M on M′	101°	30′
M on *h*	129	15
M on g	163	23
g on g′	134	45
a on *a*′	111	50
a on *b*	161	30
h on c_1	139	45
h on c_2	121	15
c_1 on c_2	161	30
c_2 on c'_2	117	30



*Sulphate of Iron.—Sulphate of Cobalt.**

The crystalline form assigned by the Abbé Haüy to sulphate of iron is *a rhomboid;* but it was, I believe, first observed by Dr. Wollaston, that its true form was *an oblique rhombic prism.* I do not find any published account of the ordinary figure of the crystals, or of the measurements of the planes; and as its form approaches very nearly to that of sulphate of cobalt, I am induced to give the measurements of both substances in reference to the annexed figure.



In sulphate of cobalt another plane sometimes appears as e_2, which measures about 124° with P. And in both these sulphates there are also other planes *a* and *e*, which occur on some of the crystals.

	Sulphate of iron.		Sulphate of cobalt.	
P on M, or M′	99°	20′	99°	45′
M on M′	82	20	82	20
P on e_1	153	00	152	45
P on e_2	123	55	122	55
P on a_1	159	00	0	0
P on a_2	136	10	135	55
P on *c*′	119	15	118	53

Chromate of Potash.

The primary form has been determined from some very perfect and brilliant crystals which I have received from M. Teschemacher, and the measurements given below have very nearly coincided on several of these.

There is a distinct cleavage parallel to the plane *h*, but apparently in no other direction. The primary form inferred from that of the crystals, as shown in fig. 1, is *a right rhombic prism.*

* For this salt I am indebted to Mr. Cooper.

Fig. 1.

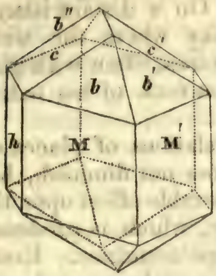


Fig. 2.

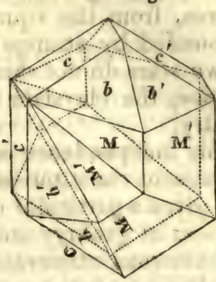


Fig. 2 represents one of the varieties of intersected crystals which occur very frequently among the single ones, the nature of which will be readily understood from the similar letters placed on the corresponding planes.

M on M′	107°	26′
M on *b* } M′ on *b*′ }	133	52
M on *h*	126	17
h on *c*	119	43
c on *c*′	120	34
c on the lateral plane *c*′ fig. 2	119	43

Article VII.

On the Constitution and Mode of Action of Volcanoes, in different Parts of the Earth. By Alexander Von Humboldt.*

When we consider the influence which scientific travels into distant regions, and a more extended geographical knowledge, have for some centuries past exerted upon the study of nature, we soon discover how this influence has varied according to the objects of inquiry, which have been, on the one hand, the forms of the organic world, and, on the other, the inanimate formation of the earth ;—the knowledge of rocks, their relative ages, and origin. Different forms of plants and animals enliven the earth in every zone, as well in the plains, where the heat of the atmosphere is determined by the geographical latitude and the different inflexions of the isothermal lines, as where it changes suddenly on the steep declivities of the mountains. Organic nature gives a peculiar physiognomical character to every zone, which is not the case with the inorganic world where the solid crust of the earth is divested of its vegetable covering. The same rocks approaching

* Read before the Royal Academy of Sciences of Berlin, Jan. 24, 1823.

to and receding from each other in groups occur in both hemispheres, from the equator to the poles. On a distant island, surrounded by strange plants, under a sky where the well-known stars do not shine, the sailor recognises, often with glad surprise, the clayslate which is the common rock of his native country.

This independence of the geognostical relations of places on the present constitution of their climate, does not diminish, but only gives a particular direction to the favourable effect upon the progress of geology and physical geognosy, which is produced by numerous observations made in foreign countries. Every expedition enriches natural history with new plants, and new genera of animals; at one time they are organic forms ranging themselves with well-known types, and representing to us, in its original perfection, a regularly woven, though often apparently interrupted texture of animated creatures; at another, they are forms which appear to be isolated, as vestiges of genera which have been destroyed, or as surprising members of groups still to be discovered. Such a variety is not presented by the examination of the solid crust of the earth; it rather reveals to us an agreement, which excites the admiration of the geognost, between the parts of which it is composed, in the superposition of masses of different natures, and in their periodical repetition.

In the chain of the Andes, as well as in the central mountains of Europe, one formation seems, as it were, to occasion the existence of another; masses of the same character assume similar forms: * mountains are formed by basalt and dolerite; steep declivities by dolomite, porphyry, and quadersandstein; bell-shaped eminences and high-vaulted domes by vitreous trachyte rich in felspar.

In the most distant zones, larger crystals, as it were by internal evolution out of the more compact texture of the greater mass, aggregate into subordinate beds, and thus frequently announce the vicinity of a new and independent formation. Thus is the whole inorganic world reflected, more or less clearly, in every mountain of considerable extent; but in order to ascertain completely the most important phænomena respecting the composition, the relative age, and the origin of the different species of rocks, observations from the most distant parts of the earth must be compared together. Problems which had appeared enigmatical to the geognost in his mother country are solved near the equator. If distant zones do not furnish new species of rocks, that is to say, unknown arrangements of simple substances, as has already been remarked, they yet teach us how to discover the great laws which are every where the same, and according to which, the different strata of the earth support each

* In an imperfect translation of this paper, which has been forwarded to the Editor from the Continent, a word here occurs which cannot be decyphered; and on account of other inaccuracies which it has been necessary to correct, unaided by the original, the translation, as now given, is not to be regarded as exact in every particular.

other, appear in the form of veins, or are elevated by elastic powers.

We need not be surprised, that, notwithstanding the great assistance which our geological information derives from inquiries, having whole countries for their object, an extensive class of phænomena (with which I venture to entertain this assembly), has been treated, during so long a period, in a confined manner; the points of comparison being more difficult, and, I might say, more troublesome to find. Whatever we believed we knew, until the end of the last century, respecting the form of volcanoes, and the action of their subterraneous forces, had been derived from two mountains of the south of Italy,—from Ætna, and from Vesuvius. The first being more accessible, and having, like all low volcanoes, more frequent eruptions, has served for a type, according to which a whole distant world,—the powerful volcanoes of Mexico, South America, and the Asiatic Islands, has been considered. Such a method recalls to our remembrance the shepherd of Virgil, who expected his narrow cottage to contain the ideal of the eternal city, imperial Rome.

A careful examination of the whole Mediterranean, and principally of its easterly islands and shores, where mankind first awakened to mental culture, and to noble feelings, might certainly have dispelled such a narrow idea of nature. Out of the deep bed of the sea, among the Sporades, rocks of trachyte have arisen, like the Azoric island, which has thrice reappeared during three centuries, the intervening periods being almost equal. Between Epidaurus and Troezene, near Methone, the Peloponnesus has a Monte Nuovo which has been described by Strabo, and seen by Dodwell, higher than the Monte Nuovo of the Campi Phlegræi, near Baia; perhaps higher than the new volcano of Xorullo in the plains of Mexico, which I have found among a thousand basaltic cones, raised out of the earth, and still smoking. In the bason of the Mediterranean Sea also, the volcanic fire bursts forth, and not only from permanent craters, from isolated mountains which preserve a lasting communication with the interior of the earth, like Stromboli, Vesuvius, and Ætna; —on Ischia, near the Epomæus, and also, as it would appear from the reports of the ancients, near Chalcis in the Lelantic plains, has lava flowed out of fissures which have suddenly opened. Besides these phænomena, which have taken place in the period of history within the narrow limits of certain traditions, and which Ritter will collect and explain in his masterly Geography, the shores of the Mediterranean contain abundant remains of more ancient igneous effects. The south of France shows, in Auvergne, a range of hills, in which *bells* of trachyte occur alternately with cones of eruption, from which currents of lava have descended. The Lombardic plain, which forms the innermost bay of the Adriatic Sea, surrounds the trachyte of the Euganean Hills, where domes of granular trachyte, of obsidian, and of pearlstone, rise, which, passing into each other, break through the Jura

limestone, but never occur in narrow streams which have flowed. Similar evidences of former revolutions may be found in many parts of the Grecian continent, and in Asia Minor, countries which will afford the geognost copious subjects for examination, when the light once returns to the land whence it first beamed over the western world—when tormented mankind ceases to sink under the savage lethargy of the Ottoman.

I mention the geographical neighbourhood of so many phænomena, in order to prove, that the bed of the Mediterranean, with all its chains of islands, might have afforded to the attentive observer, every thing that has been discovered, in latter periods, under the most varied forms, in South America, on Teneriffe, or on the Aleutian islands, near the polar regions. There were accumulated objects for observation, but tours into distant regions, and the comparison of large tracts of country within and beyond Europe, were necessary, in order to discover what was common to all these phænomena, and to learn, clearly, their dependence on each other.

By the usage of language, which often gives stability and respect to the first erroneous views of things, but often, as it were, by instinct, distinguishes the truth, we apply the term volcanic to all eruptions of subterranean and melted matter; to columns of smoke and steam, which rise sporadically out of rocks, as at Colares after the great earthquake at Lisbon; to Salsæ, or conical hills of clay which emit mud, asphaltum, and hydrogen, as those near Girgenti, in Sicily, and near Turbaco, in South America; to hot Geyser springs which rise by the pressure of elastic vapours; and, in general, to all violent powers of nature which have their seat deep in the interior of our planet. In the Spanish main of America, and in the Philippine islands, the inhabitants make a distinction between igneous and aqueous volcanoes, *vulcanes de agua y de fuego:* they apply the first name to mountains, which, during violent earthquakes, from time to time, eject subterraneous water, and with a dull noise.

Without denying the connexion between the different phænomena just mentioned, it seems advisable to give a distinct language to the physical as well as to the oryctognostic branch of geognosy; and not to apply the term volcano in one instance to a mountain that terminates in a permanent crater; and in another, to every subterranean cause of volcanic phænomena.

In the present state of the earth, the most common form of volcanic eminences is that of isolated cones; such are Vesuvius, Ætna, the Peak of Teneriffe, Tunguragna, and Cotopaxi. I have seen them of every magnitude, from the lowest hills to mountains rising to the height of 17,700 feet above the level of the sea. Besides these conical mountains, there are other craters, permanently communicating with the interior of the earth, situated upon lengthened craggy ranges of mountains, not always in the middle of their wall-like summits, but towards the end, and near their declivities. Such is Pichincha which rises

between the Pacific Ocean and the town of Quito, and which has become celebrated by Bouguer's earliest formula for the barometer; such also are the volcanoes that rise in the plain de los Pastos, at the elevation of 10,000 feet.

All these differently formed summits consist of trachyte, or trap-porphyry, a granular rock, full of cracks and fissures, and composed of glassy felspar and hornblende, but often containing in addition, augite, mica, laminar felspar, and quartz.

Where the evidence of the first eruption, and where the first scaffolding, I might say, has been entirely preserved, the isolated conical hills are surrounded by a high wall of rocks forming a circus, consisting of superposed strata; such walls, or annular surrounding masses, are called *craters of elevation;* of these very important phænomena, Leopold von Buch, the first geognost of our times, from whose works I have taken several views contained in this paper, read a remarkable account, five years ago.

The volcanoes which communicate with the atmosphere by means of craters, and the conical hills of basalt and bell-shaped trachytic hills without craters, the latter either low like Sarcouy, or high like Chimborazo, form different groups. A geographical comparison shows, in one place, small Archipelagi, or, as it were, classed systems of mountains, either with craters and currents of lava, as in the Canaries and Azores, or devoid of craters and real currents of lava in the Euganeans, and the Siebengebirge near Bonn; or it shows, in other places, single and double chains of volcanoes, connected with each other, and forming tracts of many hundred miles in length, which are either parallel to the direction of the mountains, as in Guatimala, Peru, and Java, or in directions perpendicular to their axis, as in the land of the Aztekes, where none but volcanic trachyte-mountains attain the limits of eternal snow, and those, probably, have been thrust out of a fissure nearly 500 miles in length, which divides the whole continent, from the Pacific Ocean to the Atlantic.

This aggregation of volcanoes either in single round groups, or in double ranges, affords the most determinate proof that volcanic effects do not depend upon slight causes existing near the surface of the earth, but that they are great and deeply founded phænomena. The whole eastern part of the American continent, which is poor in metals, is at present without craters, without trachyte, probably even without basalt. All the volcanoes are situated in the part opposite to Asia, in the meridian line of the Andes chain, 1800 geographical miles long; the whole of the elevated district of Quito is nothing but a single volcanic hearth, the summits of which are Pichincha, Cotopaxi, and Tunguragua. The volcanic fire now bursts forth from one, and then from another of these apertures, which we are accustomed to consider as separate volcanoes.

The progressive motion of the fire here, in the space of three centuries, turned from north to south. The earthquakes with

which this part of the world is so terribly visited, furnish remarkable evidences of the existence of subterraneous communication, not only between countries without volcanoes, as was known long ago, but even between craters which are far distant from each other. Thus the volcano of Pasto, situated to the east of the river Guaytara, uninterruptedly vomited a high column of smoke, during three months of the year 1797; and this column disappeared at the very moment, when, at the distance of nearly 300 miles, the great earthquake of Riobamba and the mud eruption of the Moya, killed from 30,000 to 40,000 Indians. The sudden appearance of the Azoric island Sabrina, on the 30th of January, 1811, was the forerunner of those dreadful shocks, which, further to the west, shook, almost uninterruptedly, from the month of May, 1811, to that of June, 1813, first the Antilles, afterwards the plains of the Ohio and the Mississippi, and at last the opposite coast of Venezuela. Thirty days after the complete destruction of the town of Caraccas, the eruption of the volcano of St. Vincent in the neighbouring Antilles took place; at the same moment when this explosion happened, on the 30th of April, 1811, a subterranean noise was heard throughout a country of 2200 geographical square miles, or 47,900 English square miles, in extent.

The inhabitants near the Apure, where it is joined by the Rio Nula, as well as those of the most distant part of the coast, compared this noise to that of artillery. From where the Rio Nula falls into the Apure, through which river I came into the Orinoco, to the volcano of St. Vincent, the distance, in a direct line, is 731 English miles. The noise just alluded to, which certainly was not communicated through the air, must, therefore, have had a deep internal cause. Its intensity on the coast of the Antillic sea was scarcely greater than in the interior of the country.

It would be useless to augment the number of examples, but for the purpose of recalling to memory a phenomenon which has become historically interesting to Europe, I will mention the earthquake at Lisbon. At the same time with this, on the 1st of November, 1755, not only were the Swiss lakes, and the sea on the Swedish shores violently agitated, but even in the easterly Antilles, around Martinique, Antigua, and Barbadoes, where the tide never exceeds 28 inches, it suddenly rose to 20 feet. All these phænomena prove, that the subterranean powers act either dynamically, by producing tension and vibration, as in earthquakes; or chemically, by producing or altering substances, as in volcanoes. They prove, likewise, that these powers do not act from superficial causes, from the exterior crust of the earth; but from deeply-seated causes, from the interior of our planet; extending their simultaneous effects to the most distant parts of the earth, through fissures and empty veins.

The more different the structure of volcanoes; that is to say, of those raised masses which surround the canal through which

the melted substances proceed from the interior of the earth to its surface, the more important is it to become thoroughly acquainted with that structure, by exact measurement. The interest attached to this measurement, which has been a particular object of my examination in another part of the world, is heightened by the consideration, that that which is to be measured is a variable magnitude. The physiognomy of nature consists in the change of phænomena tending to connect the present with the past. In order to ascertain a periodical return, or the laws of progressive natural changes in general, certain fixed points are necessary; and observations carefully made at stated periods, may serve for numerical comparison. Had the mean temperature of the atmosphere in different latitudes been observed for a few thousand years, and the mean height of the barometer at the level of the sea, we might now know in what proportion the heat of different climates has increased, or diminished, and whether the height of the atmosphere has undergone any changes. Similar points for comparison are required, for the variation and the declination of the magnetic needle, and for the intensity of the electromagnetic power, upon which two excellent philosophers of this Academy have thrown so much light. If it be a praiseworthy undertaking of learned societies to inquire assiduously into the changes of temperature undergone by the globe, into those which take place in the pressure of the atmosphere, and in the magnetic variation,—it is the duty of a travelling geognost, in ascertaining the inequality of the earth's surface, to consider, principally, the variable height of the volcanoes. What I formerly attempted on the mountains of Mexico, on the Toluca Nauhiampatepetl and Xorullo, and in the Andes of Quito, on the Pichincha, I have found opportunity, since my return to Europe, to repeat at different periods on Vesuvius. Saussure measured this mountain in 1773, at the time when both sides of the crater, the south-eastern and north-western, appeared to be of equal altitude; he found their height to be 609 toises (3894 feet) above the level of the sea. The eruption of 1794 occasioned a fall on the south side, which even the unaccustomed eye discovers at a great distance. In 1805, I measured Vesuvius three times, in conjunction with M. von Buch, and M. Gay-Lussac; we found the elevation of the northern edge, opposite to Monte Somma, la Rocca del Palo, to be exactly the same as Saussure had before determined it; the southern edge we found 71 toises (454 feet) lower than it was in 1773; the total height of the volcano on the side opposite Torre del Greco (towards which side the fire seems to have acted the most powerfully, during the last 30 years), had diminished one-ninth part.

The cone of ashes on Vesuvius bears the proportion of one-third to the height of the whole mountain, that on Pichincha is as 1 to 10, and that on the Peak of Teneriffe as 1 to 22; Vesuvius has, therefore, the largest cone of ashes in proportion,

because, probably, as a low volcano, it has acted principally through its summit. A few months ago, I succeeded not only in repeating my former measurements on Vesuvius, but also in ascertaining the elevation of all the edges of the crater. This work, perhaps, deserves some consideration, for the periods at which it was executed include those of the great eruptions from 1805 to 1822, and it is, perhaps, the only admeasurement yet published of any volcano which may be compared in all its parts. It proves that the edges of the craters, not only where they evidently consist of trachyte, as in the volcanoes of the Andes, but likewise everywhere else, are much more constant phænomena than has hitherto been believed. Simple angles of elevation ascertained from the same points are more proper for these examinations than barometrical and trigonometrical measurements. According to my last determination, the north-western edge of the crater of Vesuvius has not changed its form in the least since Saussure's time, a period of 49 years. The south-eastern edge towards Bosche tre Case, which became about 450 feet lower in 1794, has sunk very little since that time.

If in the description of great eruptions, in the public papers, the completely changed form of Vesuvius has frequently been mentioned, if this opinion often seems to be corroborated by the picturesque views of the mountain made at Naples, the cause of this mistake may be found in the circumstance, that the outlines of the edges of the crater have been confounded with those of the cone of eruption which is accidentally formed in the middle of the crater, upon a bottom that has been raised by vapours. Such a cone of eruption, consisting of rapilli and slags loosely heaped together, has become visible over the south-eastern edge of the crater, since 1816 and 1818. The eruption of February, 1822, had so much increased it that it had become from 70 to 80 feet higher than the north-eastern edge of the crater, Rocca del Palo. This remarkable cone, which, at Naples, they were accustomed to consider as the true summit of Vesuvius, fell in with a tremendous noise, during the eruption of the 22d of October, so that the bottom of the crater, which had been uninterruptedly accessible from the year 1811, now lies 850 English feet beneath the northern edge, and about 213 feet deeper than the southern edge of the volcano. The variable form and relative situation of the crater of eruption, the opening of which must not be taken for the real crater of the volcano, as frequently has been done, gives, at different times, a peculiar physiognomy to Vesuvius; and the historiographer of that volcano, from the mere outline of the summit, and the relative height of the northern or southern side of the mountain, as it is drawn in Hackert's Views in the palace of Portici, would guess the year in which the artist made the sketch of his picture.

In the night between the 23d and 24th of October, one day after the fall of the cone of slags 400 feet in height, when small

but numerous currents of lava had already flowed, the fiery eruption of ashes and rapilli began. It continued uninterruptedly for twelve days, but was most violent during the first four. During this time the detonations in the interior of the volcano were so violent, that the mere concussion of the air (no earthquake had been observed) caused the roofs to burst in the palace of Portici. In the surrounding villages of Resina, Torre del Greco, Torre del Annonciata, and Bosche tre Case, an interesting phœnomenon was observed; the atmosphere was so thickly filled with ashes, that the most intense darkness overspread the whole country for several hours in the middle of the day. The people walked in the streets with lanterns, as is often done at Quito when Pichincha is in eruption. The flight of the inhabitants was never more general; currents of lava were less feared than a fall of ashes, a phenomenon which was unknown there with such violence, and in consequence of the relations respecting the destruction of Herculaneum, Pompeii, and Stabiæ, filled the minds of the people with frightful images.

The hot steam which rose from the crater during the eruption and passed into the atmosphere, formed on cooling a thick mass of clouds, around the column of ashes and fire, 9000 feet in height. This sudden condensation of steam, and, as Gay-Lussac has shown, the very formation of the clouds, increases the electric tension. Lightnings burst forth in all directions from the column of ashes, and the rolling thunder might clearly be distinguished from the interior noise of the volcano. At no former eruption had the play of electric charges been so surprising.

On the morning of the 26th of October, a singular account was circulated, that a current of boiling water had issued from the crater, and rushed down from the cone of ashes. Monticelli, the zealous and learned observer of the volcano, soon discerned that the rumour had been occasioned by an optical deception. The supposed current of water was nothing but a dry mass of ashes, which flowed down, like quicksand, from a fissure in the superior edge of the crater. A drought, which had completely desolated the fields, preceded the eruption, but the volcanic thunderstorm occasioned, towards its termination, a very heavy and continued rain. Such a phenomenon characterizes the conclusion of an eruption in every zone. On account of the cone of ashes being generally covered with clouds during this time, and likewise because the torrents of rain are heaviest in its neighbourhood, currents of mud flow down on all sides. The affrighted peasant considers it to be water which has risen from the interior of the crater, and the deceived geognost conceives that he recognizes in it either sea-water, or mud-like volcanic productions, which are called *eruptions boueuses*, or, as the old French systematic writers termed them, products of a fiery-aqueous liquefaction.

When the summits of volcanoes (as is generally the case in the chain of the Andes), extend into the region of eternal snow, or even to double the height of Ætna, the melted snow renders the inundations amazingly frequent and destructive. They are phænomena meteorologically connected with volcanic eruptions, and are multifariously modified by the altitude of the mountains, the extent of their summits covered with eternal snow, and the calefaction of the sides of the cone of ashes; but they should never be considered as real volcanic phænomena. Subterranean lakes, in connexion with alpine rivers, are formed both on the slopes and at the feet of the mountains. When the earthquakes which precede every eruption in the chain of the Andes, shake with mighty force the entire mass of the volcano, the subterranean vaults are opened, and emit, at the same time, water, fishes, and tufa-mud. This is the singular phenomenon that furnishes the fish *pimelodes cyclopum*, which the inhabitants of the high lands of Quito call *preñadilla*, and which was described by me soon after my return. When the summit of the mountain Carguairazo, to the north of Chimborazo, and 18,000 feet high, fell, in the night between the 19th and 20th of June, 1698, the surrounding fields, to the extent of about 43 English square miles, were covered with mud and fishes. The fever which raged in the town of Ibarra, seven years before, had been ascribed to a similar eruption of fishes from the volcano Imbaburu. I recur to these facts, because they throw some light on the difference between the eruption of ashes, and that of mud-like masses of tufa and trass, which contain wood, coal, and shells.

The quantity of ashes ejected by Vesuvius in the late eruptions, like all other things which are connected with great and appalling phænomena, has been enormously exaggerated in the public papers; and two Neapolitan chemists, Vincenzo Pepe, and Giuseppe di Nobili, have affirmed, that they contain gold and silver, notwithstanding the contradiction of Monticelli and Covelli.* According to my examination, the stratum of ashes which had fallen in twelve days, towards Bosche tre Case, on the slope of the cone, where rapilli were mixed with it, was only three feet in thickness, and in the plain, it did not exceed from 15 to 18 inches. Measurements of this kind must not be made in places where the ashes have been drifted by wind, like snow, or sand, nor in those where they have been accumulated by water. The times are past in which we sought only for the marvellous in volcanic phænomena, and, like Ctesias, made the ashes of Ætna fly to the Indian peninsula. Some of the Mexican gold and silver mines are certainly in trachytic porphyry, but in the ashes of Vesuvius which I collected, and which, at my desire, have been analyzed by Henry Rose, of

* See *Annals*, v. 236.

Berlin, an excellent chemist, no traces of either metal, could be discovered.

However great may be the discrepancy between the results that I have here given, but which agree with Monticelli's more exact observations, and those which have been circulated during several months past, yet the eruption of ashes from Vesuvius, from the 24th to the 28th of October, still remains the most remarkable of which we have any certain account since the death of the elder Pliny. Its quantity, perhaps, was three times as great as that of all the ashes, collectively, which have been observed to fall, during the time in which volcanic phænomena have been attentively considered. A stratum of from 15 to 18 inches in thickness, seems at first view unimportant, if compared to the mass with which we find Pompeii to be covered; but without speaking of the torrents and inundations which certainly may have increased this mass for centuries, without renewing the violent dispute concerning the cause of the destruction of the Campanian towns, which has been carried on with so much scepticism on the other side of the Alps, it may be affirmed that the eruptions of one and the same volcano at distant periods can by no means be compared with respect to their intensity. All conclusions founded on analogy are insufficient, when the question is about quantitative proportions,—the quantity of ashes and lava, the height of the column of smoke, or the violence of the detonation.

From the geographical description of Strabo, and from an opinion of Vitruvius concerning the volcanic origin of pumice, we see that until the year in which Vespasian died, that is to say, until the eruption which overwhelmed Pompeii, Vesuvius was more like an extinguished volcano than a solfatara.

When after long rest the subterranean powers suddenly open new passages, and again break through beds of primitive rocks and of trachyte, effects must necessarily take place, for which all the phænomena subsequently observed do not afford any standard of comparison. It may be clearly seen from the well-known letter in which the younger Pliny announces the death of his uncle to Tacitus, that the recommencement of the eruptions, I might say, the awakening of the dormant volcano, began with an eruption of ashes. The same circumstance was observed at Xorullo, in Sept. 1759, when the new volcano, breaking through beds of syenite and trachyte, suddenly arose in the plain. The peasants fled, because they found in their huts, ashes that had been ejected from the fissures of the earth, which was burst in every place. Every partial eruption, in the periodical general eruptions of volcanoes terminates with a shower of ashes.

There is a passage in Pliny's letter, which shows, that the dry ashes which had fallen from the air had already attained a height of from four to five feet, in the commencement of the eruption,

and without the effect of accumulation by water. "The court which led to his [uncle's] apartment," he says, "being now almost filled with ashes and pumice, it would have been impossible for him, if he had continued there any longer, to have made his way out." In the narrow space of a court, the wind could not have had any great effect in accumulating the ashes.

I have ventured to interrupt my comparative view of volcanoes by observations solely on Vesuvius, partly on account of the great interest which the last eruption has excited, and partly because every great fall of ashes almost involuntarily reminds us of the classic ground of Pompeii and Herculaneum.* We have hitherto considered the form and the effects of those volcanoes which are in permanent communication with the interior of the earth, by means of a crater. Their summits are raised masses of trachyte and lava, intersected by numerous veins; the duration of their effects causes us to believe that they have a very stable and undisturbed structure. They possess, I may say, a more individual character, which remains the same during long periods. Neighbouring mountains often furnish completely different products, leucite-lava, and felspar-lava; obsidian, with pumice, and basaltic masses containing olivine. They belong to the newer phænomena of the earth, pass generally through all the strata of secondary rocks, and their eruptions and currents of lava are of later origin than our valleys. Their life, if I may use that expression, depends upon the manner and duration of their connexion with the interior of the earth. They often rest for centuries, suddenly take fire again, and terminate as solfataras, which emit steam, gases, and acids. Sometimes, as on the Peak of Teneriffe, their summit has already become such a depository of reproduced sulphur, while mighty currents of lava flow from the sides of the mountain, like basalt below, and above, where the pressure is less, like obsidian with pumice.

Independently of these with permanent craters, volcanic phænomena of another kind exist, which have been observed less frequently, but are principally interesting in geognosy, and remind us of the primitive world; that is to say, of the earliest revolutions of our earth. Mountains of trachyte suddenly open, eject lava and ashes, and close again, perhaps, for ever: thus was it with the mighty Antisana; and thus with the Epomæus, in Ischia, in 1302. Such an eruption sometimes takes place even in the plain, as in the high lands of Quito; in Iceland, far from Hecla; and in Eubœa, in the Lelantic fields. Many of the islands which have been raised up are owing to these temporary phænomena. In these cases the communication with the interior of the earth is not permanent, and the effect ceases as soon as the fissure, which is the communicating channel, is

* The author here mentions a paper on the data of his measurements at Vesuvius, which was unsuitable for reading; and then proceeds to notice a collection of minerals that he brought with him, and which will be added to the Royal Museum at Berlin.

closed again. The veins of basalt, dolerite, and porphyry, which, in different parts of the world, pass through every formation; and those of syenite, augite-porphyry, and amygdaloid, which are characteristic of the newest strata of the transition formation, and of the oldest rocks of the secondary strata, have probably been formed in a similar manner. In the first age of our planet, the yet liquid substances penetrated through the crust of the earth, which was every where intersected by fissures, and assumed the form of granular rocks, either in veins, or spreading over and expanding themselves in strata. The rocks strictly volcanic which the primitive ages have afforded us, have not flowed in currents like the lava of our insulated conical hills; the same mixture of augite, titaniferous iron, glassy felspar, and hornblende, may have existed at different periods, but at one time it may have approached nearer to basalt, and at others to trachyte; the chemical substances may have combined in a crystalline form, in distinct proportions, as we are taught by M. Mitscherlich's new and important labours, and by the analogy of artificial products of fire: we find that substances similarly formed have arrived at the surface of the earth in very different ways; they have either been merely raised, or protruded by temporary fissures through the older strata; that is to say, through the already oxidized surface of the earth; or they have flowed, as currents of lava, from conical hills with a permanent crater. By confounding such different phænomena together, the geognosy of volcanoes is carried back to that darkness from which a great number of comparative observations are beginning to extricate it.

The question has often been asked, What is it that burns in volcanoes? What was it that excited the heat by which earths and metals were melted? Modern chemistry answers, that the substances which melt are the metals of the earths and alkalies. The solid crust of the earth, already oxidized, separated the surrounding air with its oxygen, from the combustible unoxidized substances of the interior of our planet. The observations which have been made in mines and caves in every zone, and which, in conjunction with M. Arago, I have collected in a particular paper, demonstrate that the heat of the mass of the earth is yet much greater than the mean temperature of the atmosphere at the same place. Such a remarkable and almost generally proved fact, is closely connected with those which are proved by volcanic phænomena. Laplace has even gone so far as to endeavour to calculate the depth at which the body of the earth may be considered to be a melted mass. Whatever doubts may be entertained, notwithstanding the veneration due to so great a name, with respect to the numerical certainty of such a calculation, thus much remains probable; that all volcanic phænomena originate in a very simple cause, in

a permanent or in a variable communication between the interior and the exterior of our planet.

The pressure of elastic vapour forces the melted substances upwards through deep fissures while they are undergoing oxidation; volcanoes, if I may so speak, are intermitting springs of the earth; the liquid mixtures of metals, alkalies, and earths, which on cooling become currents of lava, flow quietly when they are raised, and find a vent. The ancients imagined, according to Plato's Phædon, that all volcanic currents of fire flowed, in a similar way, from the Periphlegeton.

It may be permitted me, perhaps, to add to these considerations one which is still more hazardous. In this interior heat of the earth, indicated by experiments with the thermometer, and by observations on volcanoes, the cause, perhaps, may be found, of one of the most wonderful phænomena which the examination of fossils presents to us. Tropical forms of animals, arboriform ferns, palms, and bamboo-like plants, lie interred in the cold north. The primitive world every where shows a distribution of organic forms at variance with the then existing nature of the climate. In order to solve this important problem, several hypotheses have been invented; as the neighbourhood of a comet, the altered inclination of the ecliptic, the increased intensity of the solar light. Neither of these has been sufficient to satisfy at once the astronomer, the natural philosopher, and the geognost. For my part, I leave the axis of the earth unaltered, as well as the light of the solar disc, by the spots on which, a celebrated astronomer has explained both the fertility and the unfruitfulness of the fields; but I believe, that in every planet, independently of its relation to a central body, and of its astronomical situation, various causes exist of the production of heat; oxidation, precipitation, and a change in the capacity of bodies; by increase of electromagnetic charge, by the opening of a communication between the interior and the exterior part of the earth.

Where the deeply cleft crust of the earth in the primitive world radiated heat from its fissures, whole countries, perhaps, could produce for centuries, palms and arborescent ferns, and sustain all the animals of the torrid zone. According to this view, to which I have already alluded in a work just published, "Essai Géognostique sur le Gissement des Roches dans les deux Hemispheres," the temperature of volcanoes would be that of the interior of the earth itself, and the same cause which now occasions such dreadful destruction, would once have occasioned, on the newly oxidated crust of the earth, upon the deeply cleft strata of rocks, the most luxuriant growth of plants in every zone.

Even if any one should be inclined to suppose, in order to explain the marvellous distribution of tropical forms in their

ancient graves, that shaggy animals of the elephant tribe now imbedded in icebergs, were once peculiar to a northern climate, and that similar forms belonging to the same primary types, like lions and lynxes, could live in very different climates, such an explanation could not, however, be extended to the products of vegetation. For reasons which the physiology of plants explains, palms, and arboriform monocotyledones cannot sustain the northern cold, and in the geological problem we here speak of, it seems difficult to me to separate plants and animals. The same explanation must be applied to both.

Towards the end of this paper, I have combined uncertain hypothetical suppositions with facts collected from the most different parts of the world The philosophical knowledge of nature rises above a mere description of nature. It does not consist in a sterile aggregation of isolated observations. It may sometimes be allowed, therefore, to the curious and ever-active mind of man, to look back upon the past, to imagine what cannot be clearly known, and to amuse himself with the ancient, and, under many forms, returning mysteries of geogony.

Article VIII.

Observations on Napthaline, with an Account of the Process by which it is obtained, and of the Mode of crystallizing it. By Mr. F. C. Chamberlain, Agent to the Chartered Gas Company.

(To the Editor of the *Annals of Philosophy.*)

SIR,

For the purpose of procuring napthaline, the coal tar formed during the preparation of carburetted hydrogen gas is to be submitted to distillation. When a fourth part of the product intended to be obtained has been distilled, it is found to consist of a volatile spirit, ammonia, and water, holding a portion of napthaline in solution; this can only be separated either by very long standing, or by another and very different kind of distillation.

By continuing the operation, a dense oil is obtained, at the bottom of which napthaline may be observed; after this it increases gradually in quantity until about half the product is distilled; if the remaining half be received as it comes over in three separate vessels, it is found that the first portion does not contain a great quantity of napthaline; from the second, little or none is obtained, even by very long standing; but the third portion contains so much napthaline that the last few gallons

sometimes become actually solid, when it has been a few hours distilled. The quantity of napthaline usually obtained is probably about five pounds from 100 gallons of the coal tar; but if the distillation be hurried towards the middle or latter end of the operation, the quantity of napthaline is much increased: may not this happen from the conversion of the oil into napthaline by the increase of temperature? The last portion of napthaline obtained is mixed with a very large quantity of sulphur.

If sulphuric acid be added to coal tar, little or no napthaline is procured; the acid probably decomposes the napthaline, for it holds but a very small quantity of it in solution.

When spirit or oil of tar obtained in making pitch, is set aside, much napthaline separates from it in a few weeks; and this effect may be more quickly produced by artificial cold; but agitation or increase of temperature readily dissolves the portion so deposited.

The napthaline is deposited from the oil in the vessels which contain it, in a semi-crystalline state, and much resembling coral in appearance, excepting that it is greyish instead of being perfectly white; by keeping, it becomes of so very dark a brown colour as to be nearly black; when large masses of it are broken the structure is frequently crystalline at the centre.

The smell of napthaline is extremely powerful and peculiar; when melted and allowed to cool gradually, it presents cells which are intersected in every direction by beautifully white and shining plates.

The napthaline separated from oil which has been twice distilled, requires a much greater heat to sublime it than that found originally in the oil; the latter melts at about 120°, and begins immediately to sublime; but if sulphuric acid be triturated with it, it requires a greater heat even than the first mentioned for sublimation; and by this process, there is obtained a mass resembling a honeycomb in appearance, owing to the peculiar arrangement of the crystalline napthaline.

During the sublimation of napthaline, a fluid is obtained which is worthy of careful examination. At first its taste is sweet, and highly aromatic; it is afterwards pungent; and occasionally hydrocyanic acid may be obtained from it, and in pretty considerable quantity.

When napthaline is mixed with water, it rises in vapour with the water; in this way it is obtained in a state of greater purity than by sublimation, but the quantity procured is small. Napthaline is soluble in spirit of wine, and by evaporation crystals are obtained, but they are neither large nor perfect; nor when dissolved in oil of tar, can any distinct crystals be obtained; after trying various fluids for the purpose of procuring perfect crystals, I succeeded but with oil of turpentine.

When napthaline is added to the last mentioned fluid, its temperature sunk from 65° to $57\frac{1}{2}$°, and the best method of causing the solution to crystallize is the following: Dissolve as much napthaline in a quart of oil of turpentine as it is capable of taking up by agitation; then add about two ounces more napthaline, and dissolve it in the oil with the assistance of heat. Set the solution in a very cool place to crystallize; in this way long prismatic crystals terminated by pyramids will be procured. If the fluid poured off after the formation of these prismatic crystals remain in a cool place, large hexagonal plates may be obtained. To obtain a honeycomb mass, differing from that formed by sublimation only in the greater thickness of the partitions, pour off the fluid from the plates obtained as above, after they have been forming for at least 48 hours; set it aside to crystallize, and in a few days, the honeycomb mass will be procured.

It is very entertaining to watch the fluid when crystallization is commencing. Minute particles are seen passing from one part of the vessel to another; sometimes a crystal will be perceived to increase suddenly in size; it will then circulate through the whole of the fluid with great rapidity, and afterwards approaching another similar crystal, they will for some time mutually attract each other; but as soon as they come in contact, both are violently repelled to a considerable distance; after a time attraction recommences; they again approach each other, and are again repelled; the repulsive power lessening after every contact, they eventually unite.

In this way several prismatic crystals may be observed undergoing alternate and mutual attraction and repulsion, and eventually forming the radii of a hexagon, which is by degrees completed, and becomes a regular hexagonal plate.

The action of nitric acid upon napthaline is peculiar; when they are triturated together, a butyraceous compound is formed, which smells exactly like new hay. If the acid be used in considerable quantity, a great number of small spiculæ, which have the appearance of a salt, are seen floating in it. It might be supposed that they are crystals of nitrate of ammonia, but this is not the case, for they are nearly tasteless, and difficultly soluble; but their true nature I have not yet determined.

Article IX.

Astronomical Observations, 1823.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

July 7. Solar eclipse .... { Beginning not observed.
End 17h 44′ 38″ Mean Time.

Clouds prevented the observation of the commencement; but the ending was made under very favourable circumstances.

Article X.

Analyses of Books.

1. *The Elements of Experimental Chemistry.* By William Henry, MD. FRS. &c. &c. The Ninth Edition, comprehending all the recent Discoveries; and illustrated with 10 Plates by Lowry, and several Engravings on Wood. In Two Volumes. 1823.

In noticing the present edition of Dr. Henry's Elements of Chemistry, it is not my intention to enter minutely into a discussion of its merits: it is a work which has been so long and justly appreciated as to bid defiance to criticism, and render particular commendation superfluous. It would, however, I think, be unjust to the author and the public, not to depart a little from the usual course in thus announcing a new edition of an established work, on account of the improvements and numerous important additions with which it has been enriched.

With true philosophical caution, Dr. Henry has not been hasty in admitting more modern doctrines to displace those which were not only by him, but by the chemical world in general, received as true, until within a few years. In making this remark, I allude to Sir H. Davy's views of the nature of chlorine; and if the late edition of Dr. Henry's work was in any respect imperfect, it appeared to me to be in the uncertainty which pervaded it with respect to the nature of chlorine, and the consequent difficulty which must have attended the learner in acquiring settled opinions, when the teacher appeared undecided.

In the present edition this indecision is removed, and although the former opinion of the compound nature of chlorine may be

learned from it, yet it is now treated of as an elementary body, and every part of the work is in unison with this doctrine.

Another great improvement has been adopted; in his views of the atomic constitution of bodies, Dr. H. has followed Dr. Prout's opinion with respect to the relative weights of the atoms of hydrogen and oxygen, viz. as 1 to 8; and in doing this, he has also admitted the consequence which results from it, that the weights of all other bodies are multiples of hydrogen by a whole number; at least his table of the weights of atoms is in agreement with this opinion, nor do these weights differ in many instances, or very materially, from those given by Dr. Thomson.

There are several parts of the present edition which, as required by the present state of chemistry, have been entirely rewritten; indeed one discovery has been made, and has constituted a highly curious and important branch of science since the publication of the former edition; I allude of course to the subject of electromagnetism: Dr. Henry has treated of it with brevity; but he has stated the leading facts of the subject as much in detail as the nature of the case would permit.

In addition to Electromagnetism, those parts of the work which are either entirely new, or remodelled, are numerous; among others I may enumerate, Corrections for Moisture in Gases, vol. i. p. 25; Deutoxide of Hydrogen, p. 262; Compounds of Carbon and Chlorine, p. 348; Hyposulphurous and Hyposulphuric Acids, &c. &c. A new arrangement of the metals has likewise been adopted.

In the second volume, the additions have also been important, particularly on the subject of the Vegetable Alkalies, Vegetable Analysis, and the Analysis of Mixed Gases. There are several parts of the work which I should like to present to the notice of the reader, and I do not know that I can select a subject which has of late excited more attention, both as a matter of science and of economy, than the nature of the combustible gases produced from the decomposition of coal and oil. With this extract I shall close the notice of a work eminently calculated to inform not only the student, but containing the newer discoveries, which those who have been long acquainted with the science are frequently prevented from acquiring in a more extended form:

"*On the Mixed Combustible Gases from moist Charcoal, Alcohol, Ether, Coal, Oil, Tallow, and Wax.*"

"The two gases, which have been just described under the names of carburetted and bicarburetted hydrogen, appear to me to be the only compounds of those elements, that have as yet been proved to be distinct and well-characterized species; though it is extremely probable, as I have shown in the Philos. Trans. for 1820, that another gas exists, which was first observed by Mr. Dalton; is heavier and more combustible than olefiant gas; and contains a larger proportion of carbon. It is of mixtures of two or more of those three gases, with occasionally a

proportion of carbonic oxide, that the almost infinite variety of aeriform products are constituted, which are obtainable by the exposure of moistened charcoal, of alcohol or ether, of oil, tallow, wax, or coal, to a heat a little above ignition. This view of the subject, at least, appears to me much more probable, than that they are so many distinct compounds of carbon and hydrogen, which, on this theory, would be capable of uniting in all possible proportions with each other.

"Of these aeriform compounds, the gases from coal and from oil are of most importance, from their widely extended use in artificial illumination.

"*Coal Gas.*—By submitting coal to distillation in an iron retort, besides a portion of tar and solution of carbonate of ammonia, which condense in a liquid form, a large quantity of permanent gas is evolved. This gas I have shown (Phil. Trans. 1808 and 1820) is extremely variable in composition and properties, not only when prepared from different coals, but from the same kind of coal under different circumstances. Within certain limits, the more quickly the heat is applied, the greater is the quantity, and the better the quality, of the gas obtained from coal; for too slow a heat expels the inflammable matter in the form of tar. The early products of gas are, also, the heaviest and most combustible, and there is a gradual decline in quality towards the close of the distillation, insomuch that the last products are inferior, by more than one half, to the first. The general name of *coal gas* is, therefore, quite indefinite. It is, in fact, a mixture of the two varieties of carburetted hydrogen, with a third which remains to be more fully investigated, as well as with hydrogen gas, carbonic oxide, carbonic acid, nitrogen, and sulphuretted hydrogen gases, in ever-varying proportions. To describe the methods of separating these gases from each other, would lead into minute details not suited to an elementary work, and I refer, therefore, to the papers which I have published in the Phil. Trans. for 1808 and 1820, and in the third volume, Second Series, of the Manchester Society's Memoirs, or *Annals of Philosophy*, vol. xv.

"Coal gas, as generally procured, has a very disagreeable odour, arising from sulphuretted hydrogen, and, perhaps, a little sulphuret of carbon; but both these may be washed out of it by cream of lime, with (as I have shown) very little loss of illuminating power, and with an entire removal of all unpleasant smell either before or during burning. The best gas has the specific gravity ·650 or upwards; and each volume consumes about $2\frac{1}{4}$ volumes of oxygen, and gives $1\frac{1}{4}$ volume of carbonic acid; the last portions have a specific gravity as low as ·340, and each volume consumes about 8-10ths of a volume of oxygen, and gives about 3-10ths of a volume of carbonic acid. In the best gas, chlorine, applied as directed, p. 416, detects from 13 to 20 per cent. of olefiant gas, and the remainder is almost pure carburetted hydrogen: but the last products contain little or no

olefiant gas, much less carburetted hydrogen, and instead of these, a large proportion of hydrogen and carbonic oxide, both of which afford very little light by their combustion.

"It is scarcely possible to assign the quantity of gas, which ought to be obtained from a given weight of coal, but it may be considered as an approach to a general average to state, that 112 lbs. of good coal are capable of giving from 450 to 500 cubic feet of gas of such quality, that half a cubic foot per hour is equivalent to a mould candle of six to the pound, burning during the same space of time.

"*Oil Gas.*—In Nicholson's Journal for 1805, I have given an account of some experiments on the gas obtained by the destructive distillation of spermaceti oil, which showed that of all the artificial gases, this, next to olefiant gas, consumes most oxygen, and is the best adapted to afford light. Since that time, an apparatus has been invented by Messrs. Taylor, of London, which has greatly facilitated the preparation of oil gas on a large scale, and this gas is now much used as a source of artificial light. The process consists in letting whale oil (the purity of which is not essential, since very inferior oil answers the purpose) fall by drops into an iron cylinder placed horizontally in a furnace, and ignited to a cherry redness. From each wine gallon of oil, about 100 cubic feet of gas may with care be obtained, of the specific gravity of more than ·900, containing upwards of 40 per cent. of gas condensible by chlorine, and of which 100 volumes consume 260 volumes of oxygen, and yield 158 of carbonic acid. But of gas from Wigan cannel, when the whole product is mingled together, 100 measures do not saturate more than 155 of oxygen, and give 88 measures of carbonic acid. Oil gas, therefore, from this document, may be inferred to contain, in a given volume, twice the quantity of combustible matter that is present in the average of gas from cannel coal; and its illuminating power will be as 2 to 1. The experiments of Mr. Brande led him to conclude, that to produce the light of ten wax candles for one hour, there will be required

2600 cubical inches of olefiant gas.
4875 .... oil gas.
13120 .... coal gas.

"But it seems probable that the coal gas, employed in these experiments, was below the general standard, and that it is a fair average to consider 1 volume of oil gas as equivalent to 2 or at most $2\frac{1}{2}$ volumes of gas from coal of good quality. This estimate agrees with the experience of the late Mr. Creighton, of Glasgow, author of the excellent article 'Gas Lights,' in the Supplement now publishing to the Encyclop. Britan. Oil gas he considers as superior, in an equal volume, to good average coal gas, in the proportion of only 2 to 1; and he has given the

following table of the comparative expence of lighting with these two gases, and with oil and tallow.

	s.	d.
Valuing the quantity of light which 1 lb. of tallow gives in candles at	1	0
An equal quantity of light from sperm. oil consumed in an Argand's lamp, will cost	0	$6\frac{1}{2}$
Ditto from whale oil gas	0	$4\frac{1}{2}$
Ditto from coal gas	0	$2\frac{3}{4}$

"Twenty cubic feet of coal gas, or ten of oil gas, he considers as equivalent to a pound of tallow, and 5000 grains of good sperm. oil to 7000 of tallow, or 1 lb. avoirdupois.

"The advantages of oil gas over gas from coal are, that smaller distilling vessels are required; that gasometers and conduit pipes of half the capacity are sufficient; that no washing apparatus is necessary; that the trouble and expence of removing waste materials is avoided; and that the gas affords a much brighter light, and with a smaller production of heat, and also of water. When only a moderate quantity of light is required; when it is an object to save room or labour; and in countries where coal is dear, oil gas is entitled to a decided preference; but it cannot be brought into competition with coal gas, where coal is cheap, or where the establishments to be lighted are of very considerable magnitude, and of such a nature as to allow of their being freely ventilated.

"Of the comparative value of different compounds of hydrogen and charcoal for the purpose of illumination, it still appears to me that the only accurate test is the one which I proposed in Nicholson's Journal for 1805, viz. the quantities of oxygen gas required to saturate equal volumes. If 100 measures, for instance, of one gas, require for perfect combustion 100 measures of oxygen, and 100 measures of another gas take 200 of oxygen, the value of the second will be double that of the first. Specific gravity, though a guide to a certain extent, is not a sufficient one, for the weight of a gas may be owing to a large proportion of carbonic oxide, which is capable of giving out only a very small quantity of light. Photometrical experiments also appear to me to require greater perfection in the instruments that have been invented for that purpose, before we can implicitly trust to results obtained by their means; but there can be no fallacy in the combustion of these gases by oxygen, if conducted with ordinary care, and especially if, in each instance, an average be taken of two or three trials, which need not occupy more than a few minutes. Nor can it admit of a doubt that, other circumstances being equal, the brilliancy of light evolved by the combustion of gases which are constituted of purely inflammable matter, will bear a proportion to their densities, perhaps even a

greater proportion than one strictly arithmetical; because, while by the combustion of denser gases a higher temperature is produced, the cooling agencies remain the same. It is probable, therefore, that of two gases, composed of the same ingredients, that which has a double density will afford somewhat more than a double quantity of light."—*Edit.*

2. *A Familiar Introduction to Crystallography; including an Explanation of the Principle and Use of the Goniometer. With an Appendix, containing the Mathematical Relations of Crystals, Rules for drawing their Figures; and an Alphabetical Arrangement of Minerals, their Synonymes, and Primary Forms. Illustrated by nearly* 400 *Engravings on Wood.* By Henry James Brooke, FRS. FLS. &c. London, 1823.

Nearly a quarter of a century has now elapsed, since the late Abbé Haüy first presented science, in a complete and systematic form, in his Traité de Minéralogie, with the results of the beautiful investigations of the geometric characters and structure of mineral substances, in which he had then for some years been engaged; and many of which he had published before in detached memoirs, inserted in the Journal des Mines, and other periodical works. Attempts had been made by various writers on mineralogy early in the last century, to confer a scientific form on the knowledge of crystallized bodies, but it is to Romé de L'isle that we are indebted for the first definite rudiments of crystallography, and likewise for the first useful application of the science to the determination of mineral species. The structure of crystals, however, appears to have been first noticed by Bergman, and Gahn, and also, about the same time, by our ingenious countryman, Mr. Keir, of Birmingham. All the subjects which had attracted the attention of these observers were pursued with astonishing industry and success by Haüy, who, by a precise determination of the different crystalline forms belonging to a considerable number of minerals, and by various philosophic general views founded upon that determination, completed the establishment of mineralogy upon a truly scientific basis; to which the great improvements and discoveries in the chemical analysis of minerals on the one hand, and the minute examination of their external characters instituted by Werner on the other, had already very efficiently contributed.

Since the first publication of Haüy's treatise, however, little progress has been made in crystallographic science, particularly in this country, while almost every other branch of natural philosophy has received the most important accessions during that period. Even at the present time, this science, comparatively speaking, has but few votaries among us, and many persons to whose pursuits a thorough acquaintance with it would seem to be

almost indispensably necessary,—chemists, writers on mineralogy, and even *professors of that science* (we speak not at random, or from doubtful authority), appear to have altogether neglected crystallography, properly so called. There is a variety of circumstances which tend to allay the surprise that might otherwise be excited by these facts, though they cannot diminish our regret that so beautiful, and at the same time so important a branch of study, should have been thus treated. Among these, the in some measure abstruse mathematical aspect in which crystallography was presented by Haüy, contrasted with the easy empirical determination and nomenclature of crystals taught in the Wernerian school, which is probably the most defective part of the system followed by its professors; and the apparently confined applicability of this science to practical purposes in the arts of life, appear to have had great effect in limiting its cultivation. It must be admitted likewise, that certain inconvenient and even unphilosophical views embraced by the method of Haüy, have also contributed to this effect.

Such then being the case, we cannot but congratulate the scientific public on the appearance of Mr. Brooke's "Familiar Introduction to Crystallography," a work, we conceive, which is calculated to be of much utility in remedying the evil to which we have just adverted. We proceed to a brief review of its contents.

It commences with a series of definitions, some of which are of a very elementary nature, so as to accommodate those who are even unacquainted with the first rudiments of geometry. These are succeeded by a particular and explanatory account of the principle and method of using both the common and the reflective goniometer. To this follows Sect. I, containing a brief general and historical view of the science of crystallography. In Sect. II, Mr. Brooke first describes the Abbé Haüy's system of molecules, and then details, nearly in the following terms, a new theory on this subject. We must omit the diagrams with which this theory is illustrated, but it is so clearly detailed, that the reader may, we think, acquire a correct knowledge of it without them.

"The very complicated system of molecules which the Abbé Haüy has, by this view of the structure of the octahedron and dodecahedron, introduced into his otherwise beautiful theory of crystals, and the apparent improbability that the molecules of the cube, the regular octahedron, tetrahedron, and dodecahedron, *among whose primary and secondary forms so perfect an identity subsists*, should really differ from each other, have induced me to propose a new theory of molecules in reference to all the classes of octahedrons, to the tetrahedrons, and the rhombic dodecahedrons, which I shall now state.

"*Fluate of lime*, as we have seen, has for its primary form a *regular octahedron*, under which it sometimes occurs in nature;

but it is generally found in the form of a *cube,* and sometimes as a *rhombic dodecahedron,* and it has *a cleavage in the direction of its primary planes.*

" *Galena,* whose primary form is *a cube,* is also found under the forms of an *octahedron,* and *rhombic dodecahedron,* with *a cleavage parallel to its cubic planes.*

" *Grey copper,* whose primary form is a *tetrahedron,* occurs under the forms of the *cube, octahedron,* and *rhombic dodecahedron.*

" *Blende* is found sometimes, though rarely, crystallized in *cubes,* sometimes in *octahedrons, tetrahedrons,* and *rhombic dodecahedrons.*

" Having thus observed that the *cube, the regular tetrahedron* and *octahedron,* and the *rhombic dodecahedron, are common as primary or secondary forms to different crystallized substances, we may reasonably infer that they are produced in each instance by molecules of a form which is common to all; and let us suppose this common molecule to be a cube.*"

Mr. Brooke here gives four diagrams, showing the arrangement of the cubic molecules in each of these forms: their arrangement in the cube may readily be conceived, without explanation; in the tetrahedron they are so arranged that the true mathematical edges of the solid are described by the diagonals of the cubic molecules which form the rude edges in such a merely approximative representation of the subject as can be presented by a diagram; the axes of the octahedron consist of the prismatic axes of its cubic molecules; the arrangement in the rhombic dodecahedron is precisely that which is commonly represented in figures showing the formation of that solid, by decrement, from a primary cube.

" These arrangements of cubic molecules," continues Mr. B. " cannot be objected to on account of any supposed imperfection of surface which would be occasioned by the faces of all the primary forms, except the cube, being constituted of the edges, or solid angles, of the molecules. For as we observe that the octahedral and dodecahedral planes of some of the secondary crystals of galena, which are obviously composed of the solid angles, or edges, of the cubic molecules, are capable of reflecting objects with great distinctness, it is evident that the size of the molecules of galena is less than the smallest perceptible inequality of the splendent surface of those planes, and hence we infer generally, that *there will be no observable difference in brilliancy between the surfaces of the planes obtained by cleavage parallel to the sides of molecules, and of those which would expose their edges or solid angles.*

" *This theory may be reconciled with the cleavages which are found to take place parallel to the primary planes of the tetrahedron, the octahedron, and the rhombic dodecahedron, as well as to those of the cube, if we suppose the cubic molecules capable of being*

held together with different degrees of attractive force in different directions. I shall call this force *molecular attraction.**

"When this attraction is *least* between the *planes* of the *molecules,* they will be more easily separated by cleavage *in the direction of their planes,* than in any other direction, and a cubic solid will be obtained.

"When the attraction is *least* in the direction of the *axis of the molecules,* they will be the most easily separated in that direction, and the *octahedron* or *tetrahedron* will be the result of cleavage.

"And if the attraction be *least* in the direction of its *diagonal planes,* the *edges* will be most easily separated, and a *rhombic dodecahedron* will be the solid produced by cleavage.

"This supposition of greater or less degree of molecular attraction in one direction of the molecule than in another, is consistent with many well-known facts in crystallography.

"The primary form both of corundum, and of carbonate of lime, is a rhomboid; and the crystals of these substances may be cleaved parallel to their primary planes, the carbonate of lime cleaving much more readily than the corundum. But the corundum may also be cleaved in a direction perpendicular to its axis, which carbonate of lime cannot be.

"This cleavage would *either divide the rhombic molecules in half,* or, *the cleavage planes would expose the terminal solid angles of the contiguous molecules.*

"But it is contrary to the nature of molecules that they should be thus divided, and we may therefore infer from this transverse cleavage that the *molecular attraction* is *comparatively less* in the direction of the perpendicular axis of the molecules of corundum, than it is in the same direction of those of carbonate of lime. And from the *greater adhesion* of the *planes of corundum,* than of *those of carbonate of lime,* we infer that the attraction is *comparatively greater* between the planes of the molecules of the corundum, than between those of carbonate of lime.†

"This supposition of the existence of a greater or less degree

* "It is possible to conceive that the nature, the number, and the particular forms, of the *elementary particles* which enter, respectively, into the composition of these three species of cubic molecules, may vary so much as to produce the variety of character which I have supposed to exist."

† "I am aware of an objection that may be made to this view of the subject, by supposing all the cleavages which are not parallel to the primary planes of a crystal, to be parallel to some secondary plane, and to be occasioned by the slight degree of adhesion which frequently subsists between the secondary planes of crystals and the plates of molecules which successively cover them during the increase of the crystal in size; but although the second set of cleavages may sometimes be connected with the previous existence of a secondary plane, it may also be explained according to the theory I have assumed."

"Those cleavage planes which would not expose the planes, edges, or solid angles of the molecules, must be considered to belong always to the class of *planes of composition,* a term which Mr. W. Phillips has applied to those cleavage planes which result from cleavages parallel to secondary planes only."

of molecular attraction in one direction of the molecule than in another, appears to explain the nature of the *two sets* of cleavages which occur in tungstate of lime: *one of these sets* is parallel to the planes of *an acute octahedron with a square base,* which we will call the primary crystal; the *other set* would produce *tangent planes* upon the terminal edges of that crystal. If we suppose the molecules to consist of *square prisms* whose *molecular attraction* is *greatest* in the direction of their *prismatic axis,* and nearly equal in the direction of their *diagonal planes,* and of their *oblique axes,* the *first set* of cleavages may be conceived to expose the *edges of the molecules,* and the *second set* to expose their *solid angles.*

"This theory may, by analogy, be extended to the form of molecules of every class of octahedron.

"For we may conceive the *molecules of all the irregular octahedrons to be parallelopipeds, whose least molecular attraction is in the direction of their diagonal planes.*

"Thus the *molecules of octahedrons with a square, a rectangular, and a rhombic base, would be square, rectangular, and rhombic prisms respectively; the dimensions of such molecules being proportional respectively to the edges of the base, and to the axis of each particular octahedron.*

"According to the view here taken, the following table will exhibit the form of the molecules belonging to each of the classes of primary forms."

The cube .... regular tetrahedron . . . ——— octahedron . . . . rhombic dodecahedron.	molecule, a cube.	
All quadrangular prisms	molecules, similar prisms.	
octahedron with a square base	molecule, a square prism	Proportional in dimensions to the edges of the base, and to the axis of each particular octahedron, respectively.
——— with a rectangular base	molecule, a rectangular prism.	
——— with a rhombic base. . .	molecule, a rhombic prism	
rhomboid	molecule, a similar rhomboid.	
hexagonal prism . .	molecule, an equilateral triangular prism.	

"Having thus advanced a new theory of molecules in opposition to one that had been long established, and possibly without a much better claim to general reception than the former theory possessed, I cannot avoid observing that the *whole theory of molecules and decrements* is to be regarded as little else than *a series of symbolic characters,* by whose assistance we are enabled to investigate and to demonstrate with greater facility the

relations between the primary and secondary forms of crystals. And under this view of the subject, we ought to divest our notions of *molecules* and *decrements*, of that absolute reality, which the manner in which it is necessary to speak of them in order to render our illustrations intelligible, seems generally to imply." (P. 43—52.)

Sect. III. relates to the Structure of Crystals; and Sect. IV. to Cleavage: in the latter, the author thus explains the relation of the tetrahedron to the octahedron, in reference to the theory of cubic molecules.

"The Abbé Hauy's theory, it will be recollected, supposes that if the tetrahedron obtained by cleavage from the octahedron, were to be successively reduced to an octahedron, and four still smaller tetrahedrons, we should at length arrive at a tetrahedron consisting of four single tetrahedral molecules *enclosing only an octahedral space, instead of an octahedral solid.*

"But according to the structure assigned to the octahedron by the theory of cubic molecules, that figure is an entire solid; and the smallest tetrahedron that can be imagined to exist, will contain an octahedral solid, and would be reduced to an octahedron by the removal of four *cubic* molecules from its four solid angles, and not of four *tetrahedrons.*

"Thus the necessity of adopting the tetrahedron as the molecule of the octahedron is removed, and in consequence a more simple theory of the structure of the octahedron, may be substituted for that which has been established upon the adoption of tetrahedral molecules.

"By a similar mode of reasoning, the compatibility of the cubic molecule with the solids obtained by cleavage from the rhombic dodecahedron, might be shown; and by adopting the cubic molecule, a more simple theory of decrement, in relation to the rhombic dodecahedron, may be substituted for that which has been established upon the assumption of the irregular tetrahedron as the integrant molecule, and the obtuse rhomboid as the subtractive molecule." (P. 65, 66.)

Sect. V. is allotted to the explanation of Decrements; Sect. VI. to Symmetry; and Sect. VII. to Primary Forms.

"The *derivative* or *parent* form," observes the author, "from which the *secondary* forms of any crystallized mineral may be conceived to be derived by the operation of certain laws of decrement, has been denominated the *primary form* of such mineral.

"It may be added that the *primary form of a mineral should not be inconsistent with its known cleavages, and it should generally be such also as would produce the secondary forms of those species to which it belongs by the fewest and simplest laws of decrement.**

* "The term *primary*, so defined, is merely relative, being used in contradistinction to *secondary*. It appears therefore preferable to the term *primitive*, which has been

"It is for the sake of rendering our notions of a primary form more precise, that we give this limiting, and in some degree arbitrary, definition of the term. Our purpose throughout this treatise is, to find the shortest and most direct road from the *secondary crystal* to the *mineral species* to which it belongs.

"But as we must travel first from the *secondary* to the *primary* form, it is essential that our ideas of that figure which we agree to call the primary form, should be as precise as possible."(P. 79.)

In Sect. VIII. Secondary Forms are briefly considered, in a general manner; in Sect. IX. Hemitrope and Intersected Crystals are described; and Sect. X. defines Epigene and Pseudomorphous Crystals.

In Sect. XI. are described the nature and use of the tables of modifications of the primary form which succeed it.

"In these tables," the author says, "*not merely the observed modifications of crystals, but all the numerous modifications of which each class of primary form is susceptible, while influenced by the law of symmetry, are reduced into classes, and arranged in an orderly series;* and I have added some of the *observed* instances of departure from this law, in the production of peculiar and anomalous secondary forms." (P. 98.)

"The figures of the primary and secondary forms given in the following tables, are not to be regarded as representations of crystalline forms of any particular minerals, but as exhibiting a type, or general character, of each of the classes of primary forms, and of the modifications belonging to each of those classes." (P. 101.)

These tables exhibit 150 classes of the modifications of the following 15 primary forms: the cube, regular tetrahedron, regular octahedron, rhombic dodecahedron, octahedron with a square base, octahedron with a rectangular base, octahedron with a rhombic base, right square prism, right rectangular prism, rhombic prism, oblique-angled prism, oblique rhombic prism, doubly oblique prism, hexagonal prism, rhomboid. They are followed by a table exhibiting the relations to the primary forms, of those secondary forms of crystals which are similar to some of the classes of the primary, and also of those which, when complete, are different from all the primary forms.

Sect. XII. treats of the Application of the Tables of Modifications. In Sect. XIII. (numbered XIV. by mistake), the Use of Symbols for describing the secondary forms of crystals is explained. To this section succeeds a series of tables, which terminate this part of the work, showing the Relation of the Laws of Decrement to the different Classes of Modifications.

A large portion of the Appendix consists of an outline of the method of applying the theory of decrements to determine the relations between the secondary and primary forms of crystals. Abbé Haüy has used plane trigonometry in his calculation of the

generally used to designate this original or parent form, and which seems to imply something more intrinsic, and absolute, than is required by the science into which it is introduced."

laws of decrement, but Mr. Brooke, at the recommendation of Mr. Levy, has substituted spherical trigonometry for it in this section.

To this outline succeeds a section on the direct determination of the laws of decrement from the parallelism of the secondary edges of crystals, according to the methods pointed out by Haüy, Monteiro, and Levy. A section follows, on the Methods of Drawing the Figures of Crystals, some of the examples in which are particularly elegant; and a short essay on Mineralogical Arrangement, with an *Alphabetical* Arrangement of Minerals, their Synonymes and Primary Forms, terminate the volume.

We intended to discuss in this place certain arguments employed by Mr. Brooke, respecting the difficulties of mineralogical arrangement, which we conceive to be somewhat fallacious, as well as to examine in what respect his *abecedarium* of mineralogy is really preferable to such arrangements as have a more natural character. We also intended to offer a few remarks on certain subjects of mineralogical chemistry adverted to in the list of minerals; but as we have now neither space nor time for the necessary extension of this article, we must leave all these subjects to the discernment of Mr. Brooke's readers; at the same time strongly recommending the work in general to their careful study, as the only comprehensive treatise on Crystallography which has yet appeared, in this country at least. We will conclude with the important statement given in the section on Arrangement, respecting Dr. Brewster's preference of the optical characters of minerals, as the surest means of determining their species.

"Dr. Brewster has, with that attachment which we usually evince towards a favourite pursuit, given a preference to the optical characters of minerals, as the surest means of determining their species. See a memoir by Dr. Brewster in the Edinb. Phil. Journ. vol. vii. p. 12.

"This memoir relates to a difference in the optical characters of the Apophyllites from different localities, upon which Dr. Brewster proposes to erect a particular variety into a new species under the name of Tesselite. Berzelius, as it appears from a paper, preceding that by Dr. Brewster, in the same volume of the Journal, has, at Dr. Brewster's desire, analysed the Tesselite, and found it agreeing perfectly in its chemical composition with the Apophyllites from other places. Chemically, therefore, the Tesselite does not appear a distinct species.

"A few days before Dr. Brewster's paper was published, it happened that I had been measuring the angles of the Apophyllites from most of the localities in which they occur, all of which I found to agree with each other more nearly than different minerals of the same species frequently do. The Tesselite is not therefore, crystallographically, a separate species.* But when

* "I have found several crystals of this substance corresponding in a remarkable manner in their general form of flattened four-sided prisms, terminated by four-sided pyramids with truncated summits, *but with their corresponding planes dissimilar.* The

chemistry and crystallography concur so perfectly as they do in this instance, in determining the species, to which a mineral belongs, it will be difficult to admit a variation of optical character, as a sufficient ground to alter that determination.

"A paragraph published by Dr. Brewster in the sixth volume of the same Journal, p. 183, relative to the crystalline form of the *sulphato-tri-carbonate* of lead, furnishes an additional motive to believe that the connexion between the optical characters of minerals and their crystalline forms is not yet sufficiently understood.

"Dr. Brewster admits what I believe is not liable to question, that '*the crystals of this substance are acute rhomboids.*' But he adds, 'Upon examining their optical structure, I find that they have two axes of double refraction, the principal one of which is coincident with the axis of the rhomb. The sulphato-tri-carbonate, therefore, *cannot have the acute rhomboid for its primitive form, but must belong to the prismatic system of Mohs.*'

"But it appears from the 'Outline of Prof. Moh's new System of Crystallography,' published in the third volume of the same Journal, that *a rhomboid cannot belong to his prismatic system.* For it is stated in p. 173, that '*The rhomboid, and the four-sided oblique-based pyramid,*' (the fundamental form of the prismatic system) '*are forms which cannot by any means be derived from each other; the* (*two*) *groups of simple forms, as well as their combinations, must each be always distinct from* (*the*) *other.*'

"If therefore in the hands of Dr. Brewster," Mr. Brooke justly concludes, "the use of optical characters cannot at present be relied upon for the determination of a mineral species, it may be doubted whether they can be successfully employed by less accurate and less intelligent observers."—B.

Article XI.

Proceedings of Philosophical Societies.

LINNEAN SOCIETY.

May 24.—This being the anniversary of the Society, the election of the Council and Officers for the ensuing year took place; when the following gentlemen were chosen.

Council.—James Ebenezer Bicheno, Esq.; Edward Rudge, Esq.; Joseph Sabine, Esq.; Robert Brown, Esq.; John George

planes which appear as the summits of some of these prisms, being only the *lateral planes of very short and otherwise disproportioned crystals;* so that a line passing through these, in the direction of their greatest length, would in fact be perpendicular to the axis of the primary form. Sections perpendicular to the axes of these apparently similar prisms would certainly present very different optical phenomena. But it is not probable that the practised eye of Dr. Brewster should have been misled by their apparent similarity; and the differences he has observed will still remain to be explained."

Children, Esq.; Adrian Hardy Haworth, Esq.; William Sharp Mac Leay, Esq.; Joseph Smith, Esq.

President.—Sir James Edward Smith.

Vice Presidents.—Samuel, Lord Bishop of Carlisle; Aylmer Bourke Lambert, Esq.; Edward, Lord Stanley; William George Maton, MD.

Treasurer.—Edward Forster, Esq.

Secretary.—Alexander Mac Leay, Esq.

Under Secretary.—Mr. Richard Taylor.

The following rare plants were exhibited in flower: *Pancratium Amancaes,* from the garden of the Horticultural Society; *Hyacinthus amethystinus, Polygala amara, Ranunculus Parnassifolius,* and *Braya alpina,* from the Botanic Garden at Chelsea.

June 3.—At this meeting, the following papers were read:

Description of a new Species of *Erythrina* called E. *poianthes.* By Felix de Avellar Brotero, Professor of Botany at Coimbra, For. Mem. LS.

E. foliis ternatis; foliolis lateralibus ovatis, intermedio rhombeo-ovato; omnibus subtus pubescentibus, rachi petioloque communi, aculeatis, caule arboreo aculeato, calyce obliquè truncato: latere superiori vel fisso vel integro, staminibus diadelphis vexillo vix brevioribus.

Cultivated in the Royal Botanic Garden near Lisbon, and elsewhere in Portugal. Native country unknown; probably America.

A Letter from the Rev. Mr. Whitear of Harleston, in Norfolk, stating, that the Little Bustard (*Otis tetrax*), a native of warm climates, stated by Temminck never to be found in the north, had been killed at Butley, near Orford, in Suffolk, in January last. The specimen is now in the possession of Mr. Seaman, of Ipswich.

An Extract of a Letter from the Rev. S. L. Jacob to W. G. Maton, VPLS. stating that a Flying Fish (*Exocœtus volans*) had been caught in July last, in the Bristol Channel, ten miles from Bridgewater.

A Letter from Mr. Robert Anstis relative to a bird shot in the neighbourhood of Bridgewater, varying but little from the Crested Cormorant, and distinguished by having 16 feathers in its tail. Col. Montagu had invariably found, it was remarked, that the tail of the Shag consisted of 12 feathers, and that of the Cormorant of 14.

June 17.—The following communications were read:

Description of *Antilope Quadricornis,* the *Chikara* of Bengal. By Major-Gen. T. Hardwicke, FLS.

This animal is not scarce in India, Gen. Hardwicke observed, yet it does not appear to have been hitherto particularly described. It inhabits the forests and hilly tracts of the western parts of Bengal, Bahar, and Orissa. In size, it resembles the harnessed antelope, *A. Scripta;* height about 20 inches; length,

exclusive of the tail, 33 inches; length of the tail, 5 inches; greatest circumference of the body, 29 inches. The superior or common horns are placed on the forehead, and are four or five inches in length, slightly diverging, subulate, conical, and a little directed forwards; the spurious horns are placed between the eyes, are less than the superior, and slightly diverging. The upper parts of the body are of a bright bay colour; the under parts dusky white, with a few yellow hairs. Such were the characters of the male specimen described: the female has no horns, and is less bright in colour; this distinction in colour appears to be permanent, for it continued, during four years, in a pair possessed by the author: they bred during this period, two at a birth, and the young were similarly distinguished in colour. The male was very fierce in the rutting season, and though partly domesticated, continued to be so; at this time the feeder could only approach the verge of the circle which the rope securing the animal permitted him to describe.

Description of *Buceros.*—Hornbill without the helmet or rostral appendage, with a pendant gular sac, or pouch. By the same.

The length of this bird was 36½ inches, of which the tail measured 12 inches, and the bill 7 inches; the distance between the extremities of its wings, when spread, was 53 inches; the regular appendage, marked with yellow vertical lines, and with a bright blue mark, was 3½ inches long, and 3 inches wide: weight of the bird, when living, five pounds and a quarter. The eyes large, surrounded with a naked circle, and with some bristly feathers, the pupil large and black; the irides marked with four concentric circles, of different widths, and of the following colours respectively, reckoning from the innermost, white, brown, orange, and black. The auditory apertures behind the eyes, circular, concealed when the feathers are in their natural position, but plainly visible when they are turned up.

Plumage of the body black, with shades of olive-green when viewed in a strong light. This bird is a native of the woods about Chittagong and Sylhet; and resembles the *Calao Javan* of Le Vaillant, as described by Shaw: the specimen described in this paper lived two years caged, and died while moulting.

The reading of Dr. Hamilton's Commentary on the second part of the *Hortus Malabaricus,* was continued.

In this elaborate commentary, Dr. Hamilton traces the plants described by Rheede, in the second part of the Hortus Malabaricus, through the works of succeeding writers, down to Linnæus and later botanists; giving their various synonyma, and comparing their characters as described by the different authors; occasionally suggesting new appropriations of the names in the H. M. and showing, that in some instances several species described in that work have been erroneously confounded together as one; while in others one real species has been divided into

several. Dr. Hamilton's Commentary on the first part of Rheede's great work, has already appeared in the Transactions of the Linnean Society, vol. xiii. part. ii. p. 474.

The Society then adjourned to the 4th of November next.

GEOLOGICAL SOCIETY.

May 16.—A letter was read, from Henry Heuland, Esq. For. Sec. Geol. Soc. addressed to the President, "On the Matrix of the Diamond."

In this letter Mr. Heuland describes two specimens which he laid upon the table of the Society. The first of these, from Abbaete in Brazil, was a conglomerate of oxide of iron, with small waterworn quartz pebbles, containing a diamond. This, which is called Cascalhao, Mr. Heuland believes to be of alluvial origin. The other specimen from Pereira, in Brazil, which Mr. Heuland received from Baron d'Eschwege, was a very small brilliant dodecahedral diamond, surrounded by skorodite or cupreous arseniate of iron in a gaugue or matrix of massive oxide of iron (Werner's brown ironstone.) This oxide of iron, according to Baron d'Eschwege and Alexander Caldcleugh, Esq. forms veins or beds 25 feet deep resting on chlorite schist in the mountains near Pereira. That it is the true matrix, of at least the Brazilian diamond, appears confirmed by the locality where diamonds have not before been discovered, by its being accompanied by the arseniate of copper, and by the difference of this oxide of iron from that in the Cascalhao, which is either earthy, granular, or in water-worn particles.

June 6.—A paper was read containing remarks on Sections presented by the Rivers Isla, Melgum, Proson, and S. Esk, in the County of Forfar, with some general Observations on the Geology of that County, accompanied with specimens. By Charles Lyell, Esq. Sec. GS.

The country which formed the principal subject of this communication is situated on the southern flank of the Grampians; it is occupied by old red sandstone, greywacke, and argillaceous schist, with their associated porphyries. The strata are clearly exposed by the rivers that cut through them. They are very highly inclined, and dip for the most part towards the south.

The old red sandstone may be described as consisting of two formations of sandstone, with a formation of conglomerate of great thickness interposed between them. An extensive formation of felspar porphyry occurs in the lower part of the conglomerate, and it is from the broken and rolled fragments of this porphyry that the conglomerate is for the most part composed. Between the porphyry and the conglomerate, a rock prevails of a mixed character, which seems intermediate between the two, and which it is difficult to describe or account for. The lower red sandstone, which is beneath the conglomerate, is in many parts seen to be traversed by a mass or dyke of greenstone,

which passes into serpentine, in which form it continues through a great part of its course; it lies parallel with the strata. The lower red sandstone, which is for the most part schistose, and not of great thickness, alternates with greywacke at its juncture, and the greywacke with argillaceous schist. A large mass of porphyry resembling that of the elvans of Cornwall, intersects in one part of the district the superior beds of the greywacke formation. The paper concludes with some observations on the primary rocks of the Grampians in the county of Forfar.

June 20.—The following papers were read:

A Notice on some Fossil Bones of an Icthyosaurus from the Lias near Bristol; also on two new Species of Fossil Teeth. By George Cumberland, Esq. Hon. Mem. GS.

A Letter accompanying some Specimens from Stonehenge. By Godfrey Higgins, Esq.

An Extract of a Letter from Lieut. J. Short, RE. addressed to, and communicated by, Dr. Babington, Pres. GS. containing some remarks on the Isle of Bourbon.

The Isle of Bourbon, which is situated about 120 miles from the Mauritius, and is 150 miles in circumference, appears to be chiefly of volcanic composition. An active volcano still exists. Although beneath the tropics, perpetual snow and ice cover the summits of some of the mountains which rise to an elevation of 10,000 feet. Lieut. Short observed basaltic columns of great height exposed in some parts of the island, and found olivine, lava, zeolite, and puzzolana, abounding throughout the rocks.

A Notice respecting the Pebbles in the Bed of Clay which covers the new red Sandstone in the SW of Lancashire. By John Bostock, MD. VPGS.

Article XII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Dr. Wollaston's Method of detecting Magnesia, on the smallest Scale.*

Dissolve in a watch-glass, at a gentle heat, a minute fragment of the mineral suspected to contain magnesia, dolomite for example, in a few drops of dilute muriatic acid; to this solution, add oxalic acid, to render the lime that may be present insoluble; then pour in a few drops of a solution of phosphate of ammonia or soda. Allow the precipitate to settle for a few seconds, and decant a drop or two of the supernatant clear liquid on a slip of window-glass; on mixing with this liquid two or three drops of a solution of the scentless carbonate of ammonia, an effervescence takes place; draw off to one side with a glass rod, a little of the clear solution, and trace across it, with the pressure of a point of glass or platina, any lines or letters on the glass plane; on exposing this to the gentlest possible heat (as making a little

warm water flow over it), white traces will be perceived wherever the point was applied. These consist of the triple phosphate of ammonia and magnesia. In the application of this process on the larger scale, the carbonate of ammonia should be added first, which prevents the chance of any simple phosphate of the earth being formed.—Journal of Science, &c. xv. 336.

II. *Phosphate of Uranium.*

I am indebted to the Rev. J. J. Conybeare for the information that the existence of phosphoric acid in uranite which I supposed I had first discovered was ascertained several years since. The fact, he informs me, is mentioned in a work entitled, "Elemens de Mineralogie et de Geologie," &c. Par E. M. J. Patrin. Paris, 1803.

Having never seen this work, Mr. Conybeare has been so good as to favour me with the following extract from it: "Ekebert [Ekeberg] fait sur l'uranite une observation qui serait tres curieuse si elle etait confirmée; c'est que l'acide phosphorique se trouve joint à l'oxide d'uranite. Il dit dans une note de son Mémoire sur la Phosphate de Chaux (Annales de Chimie, No. 96, p. 233), que si dans une dissolution d'uranite par l'acide nitrique on verse de l'acetite de plomb; il se fait un precipité qui est un phosphate de plomb qui fondu au chalumeau donne un polyèdre de couleur laiteuse."—Patrin, t. iv. p. 48.

Mr. Conybeare justly observes to me, that "the circumstance of Ekeberg's discovery being mentioned in a paper *not* on uranite, but on phosphate of lime, will account for its escaping the notice of Berzelius, of yourself, and even of so many professed compilers of mineralogical systems."—*Edit.*

III. *On the Use of the Electrical Faculty of the Torpedo.* By Mr. Jonathan Couch.

The following suggestion on this subject has been made by Mr. Jonathan Couch in a paper on the Natural History of Fishes found in Cornwall, printed in the newly-published part of the Transactions of the Linnean Society; vol. xiv. p. 89:—

"Torpedo or Cramp Ray. *Raia Torpedo.*—This fish is extremely rare. The numbing power of the torpedo has been much illustrated by the discoveries which have been made in galvanism; but the cause of this phenomenon appears to me not to have been explained. I would, therefore, suggest the following observations on this subject. It has been supposed, that by this faculty the torpedo is enabled the more readily to secure its prey; and when Pennant took a surmullet from the stomach of a torpedo, he concluded that it must have been first disabled by the shock before it could have been swallowed by its enemy. But I have known a lobster, whose agility is much superior to that of a surmullet, taken from the stomach of a skate; which fish possesses no such formidable means of disabling its prey. Without denying that the torpedo may devour that which it disables by the shock, I conceive that the principal use of this power has a reference to the functions of digestion. It is well known that an effect of lightning, or the electric shock, is to deprive animated bodies very suddenly of their irritability; and that thereby they are rendered more readily disposed to pass into a state of dissolution than they would otherwise be; in which condition the digestive powers of the stomach can be much more speedily

and effectually exerted on them. If any creature may seem to require such a preparation of its food more than another, it is the torpedo, the whole intestinal canal of which is not more than half as long as the stomach."

Article XIII.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Sketches of the Philosophy of Apparitions; or, an Attempt to trace such Illusions to their Physical Causes. By Dr. Samuel Hibbert. 12mo.

Lectures on the Philosophy of History, Vols. 5 and 6, bringing it down to the Revolution. By Dr. G. Miller.

An Introduction to Lamarck's Arrangement of the Genera of Shells. 8vo. By Mr. Charles Dubois.

A new Edition of Berthollet on Dyeing, with Notes and Illustrations. By Dr. A. Ure. 2 Vols. 8vo.

A Manual of Pharmacy. By W. T. Brande, Esq. Small 8vo.

JUST PUBLISHED.

Dodsley's Annual Register, or a View of the History, Politics, and Literature of the year 1822. 8vo. 16*s.* Boards.

Memoir of John Aikin, MD. By Lucy Aikin. With a Selection of his Miscellaneous Pieces, Biographical, Moral, and Critical. 2 Vols. 8vo. With a fine Portrait. 1*l.* 4*s.* Boards.

Le Bulletin General et Universel des Annonces et des Nouvelles Scientifiques; dédié aux Savans de tous les Pays et à la Libraire Nationale et Etrangère, et publié sous la Direction de M. le Baron de Ferussac. Numbers 1 to 6 are published. Subscription for one Year, or 12 Numbers, 2*l.* 2*s.*

Observations made during a Residence in the Tarentaise and various Parts of the Grecian and Pennine Alps, in Savoy, and in Switzerland and Auvergne, in 1820, 1821, 1822. By R. Bakewell. 2 Vols. 8vo. With Plates. 1*l.* 6*s.*

Jewish, Oriental, and Classical Antiquities; containing Illustrations of the Scriptures and Classical Records from Oriental Sources. 8vo. 12*s.* Boards.

The Naval History of Great Britain, from the Declaration of War by France to the Accession of George IV. Vol. I. Part II. 8vo. 14*s.* Boards. Two more Volumes are in great Forwardness, and will complete the Work.

On the Stratification of Alluvial Deposits, and the Crystallization of calcareous Stalactites. In a Letter to Dr. John Macculloch. By H. R. Oswald. 8vo. 1*s.* 6*d.*

Article XIV.

NEW PATENTS.

Edward Ollerenshaw, Manchester, hat-manufacturer, for a method of dressing and furnishing hats, by means of certain machinery and implements to be used and applied thereto.—May 27.

T. Peel, Esq. Manchester, for a rotatory-engine, for communicating motion by means of steam or other gaseous media.—May 27.

S. Wilson, Esq. of Streatham, Surrey, for certain improvements in machinery for weaving and winding.—May 31.

J. Mills, St. Clement Danes, Middlesex, and Silver-street, London, and Herman William Fairman, Silver-street, London, merchants, for certain improvements in rendering leather, linen, flax, sail-cloth, and certain other articles, water-proof.—May 31.

R. Badnall, Leek, Staffordshire, silk-manufacturer, for certain improvements in dyeing.—June 3.

T. Attwood, Birmingham, banker, for certain improvements in the making of cylinders for the printing of cottons, calicos, and other articles.—June 3.

T. Mills, Dudbridge, Gloucestershire, cloth-dresser, for certain improvements on machines for shearing or cropping woollen cloths.—June 3.

J. Perkins, Fleet-street, engineer, for certain improvements in steam-engines.—June 5.

E. Cowper, Kennington, mechanist, for certain improvements in machines and apparatus for printing calico, linen, silk, wool, paper, and other substances capable of receiving printed impressions.—June 10.

R. Mushet, Royal Mint, Tower-hill, Gent. for improving the quality of copper and alloyed copper, applicable to the sheathing of ships, and other purposes.—June 14.

R. Pew, Esq. Sherborne, Dorsetshire, for a new composition for covering houses and other buildings.—June 17.

C. Mackintosh, Esq. Crossbasket, Lanark, for a process and manufacture whereby the texture of hemp, flax, wool, cotton, and silk, and also leather, paper, and other substances, may be rendered impervious to water and air.—June 17.

J. Smith, Droitwich, Worcestershire, civil-engineer, for an apparatus for applying steam to the boiling and concentration of solutions in general, crystallising the muriate of soda from brines containing that salt, melting and refining of tallow and oils, boiling of sugar, distilling, and other similar purposes.—July 19.

J. M. Willoughby, Fair-street, Horsleydown, Surrey, Gent. for certain improvements in the construction of vessels, so as to enable them to sail with greater velocity.—June 26.

J. Green, Mansfield, Nottinghamshire, whitesmith, for certain machines used for roving, spinning, and twisting cotton, flax, silk, wool, or other fibrous substances.—June 26.

W. Vere, Crown-row, Mild-end Old-town, engineer, and H. S. Crane, Stratford, manufacturing chemist, for their improvements in the manufacture of inflammable gas.—June 30.

Article XV.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
6th Mon.								
June 1	N E	30·25	30·02	78	55	—		
2	S W	30·02	29·73	77	46	—	35	
3	N W	29·73	29·55	65	43	—	01	
4	S W	29·63	29·55	61	43	—	17	
5	S W	29·92	29·63	66	38	—	13	
6	N	30·19	29·92	70	41	·85		
7	W	30·19	30·04	69	50	—	01	
8	W	30·04	30·02	68	50	—		
9	N W	30·06	30·01	66	43	—		
10	N W	30·12	30·06	64	42	—		
11	N E	30·17	30·12	70	39	·67		
12	N E	30·12	30·00	73	42	—		
13	N	30·00	29·94	79	53	—		
14	N	30·07	29·98	76	52	—		
15	N E	30·36	30·07	72	38	—		
16	N E	30·39	30·36	71	41	·71		
17	N W	30·36	30·31	74	44	—		
18	N	30·31	30·25	60	43	—		
19	N E	30·25	30·14	71	48	—		
20	N E	30·17	30·13	72	50	—		
21	N	30·24	30·17	62	46	—		
22	N	80·24	30·17	55	41	—		
23	N W	30·17	30·07	67	40	·87		
24	N W	30·07	29·88	70	43	—		
25	S W	29·88	29·80	72	46	—	08	
26	Var.	29·80	29·45	70	52	—	15	
27	Var.	29·45	29·39	71	50	—	64	
28	S W	29·82	29·44	67	50	—	05	
29	S W	30·07	29·82	71	46	—	29	
30	W	30·07	30·01	72	44	·90		
		30·39	29·39	79	38	4·00	1·88	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Sixth Month.—1. Fine. 2. Cloudy: rain in the evening. 3. Cloudy: a shower of hail in the afternoon. 4. Showery. 5. Showery: a heavy storm of thunder about three, p. m. with large hail, and very vivid lightning. 6—13. Fine. 14. Cloudy, and fine at intervals. 15—17. Fine. 18. Fine: overcast. 19, 20. Fine. 21, 22. Overcast. 23. Fine. 24. Overcast. 25. Cloudy: some showers in the night. 26. Cloudy. 27. Showery, till about five o'clock, p. m. when there was a very heavy storm of thunder, and rain and hail of considerable size: the lightning vivid, and thunder near and frequent, the wind going round to the W. 28. Cloudy and fine. 29. Showery: a violent storm of hail about three, p. m. with thunder, followed by rain: the hail as large as peas. 30. Calm: overcast.

RESULTS.

Winds: N, 5; NE, 7; SW, 6; W, 3; NW, 6; Var. 2.

Barometer: Mean height

For the month.. 30·002 inches.

For the lunar period, ending the 2d .... 29·993

For 15 days, ending the 2d (moon south).. .... 29·994

For 12 days, ending the 14th (moon north) .... 29·961

For 15 days, ending the 29th (moon south) .... 30·033

Thermometer: Mean height

For the month.. 57·300°

For the lunar period .... 57·300

For 31 days, the sun in Gemini .... 57·133

Evaporation.. 4·09 in.

Rain. .... 1·88

Laboratory, Stratford, Seventh Month, 10, 1823. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1823.

Article I.

An Abridged Translation of M. Ramond's Instructions for the Application of the Barometer to the Measurement of Heights, with a Selection from his Tables for facilitating those Operations, reduced (where necessary) to English Measures. By Baden Powell, MA. of Oriel College, Oxford.

(*Continued from* p. 111.)

It is from the publication of the excellent work of M. De Luc on the Modifications of the Atmosphere, that we are to date the commencement of those observations which have really contributed to the advancement of our knowledge. With these subsequent additions, a work on the mode of using meteorological instruments would assuredly be highly useful, for the experimental part of the science has been far from keeping pace with the mathematical; and the art of making good observations is as yet far from being carried to the same perfection as that of employing them when made.

It is, therefore, not useless to apprize those to whom these instructions are addressed, that if exactness is the first requisite in a good observation considered in itself, there are also other conditions to be fulfilled, in order to appropriate that observation to the particular purpose in view: that both meteorology and barometrical measurements require the choice of opportune conjunctures: that the laws of general physics lay down the principles, but do not always point out their applications: that these are not within the province of dogmatical instruction; that in order to effect happy applications, there must be much skill,

discrimination, and experience employed; that we do not learn all this as we learn to read, to calculate, or to translate the terms of a formula. Let us read then, and reflect upon the works of De Luc, Saussure, the memoirs of Pictet, the books of philosophers who have devoted themselves to the scientific employment of meteorological instruments; and above all, let us continue our own observations for a long time before we trust to them, and a still longer time before we yield to the temptation of drawing inferences from them; for the goodness of observations depends on a multitude of minute precautions which habit alone renders constantly present to the memory; and the validity of the inferences rests on considerations too numerous to be at the disposal of a mind not rendered familiar to them by time and practice.

Sedentary Observations for determining the Mean Pressure at a particular Place.*

I commence with considering the method of conducting observations of the stationary kind for determining the mean pressure of the atmosphere at any place; for these form one of the principal foundations on which the science of meteorology rests, and above all because they are the means of affording marked periods for the operations of levelling.

Sedentary observations have a very limited utility if they are not comparable; that is to say, if equal pressures are not constantly expressed by equal heights of the mercurial column. We may readily conceive that this fundamental condition is but ill fulfilled by the rude instruments with which the generality of observers content themselves, and often not even by the more costly ones with which the cabinets of the curious are adorned.

It must be presumed then that the observer has a barometer carefully constructed: the mercury perfectly pure and well boiled; the tube as free from air and moisture as it can be rendered: the scale leaving nothing to be desired on the score of accuracy; he believes himself to possess a very good barometer. It is doubtless so far good; but this is not enough; and the chief condition is not fulfilled unless we are assured that it is *comparable*. What is the interior diameter of its tube? What means have been taken to correct the effects of capillarity? Is it of the siphon or cistern construction? What precautions have been taken to secure a constant level, and an invariable point of departure? Such are some few of the questions which must be satisfactorily answered before the instrument can be allowed to inspire confidence.

Let us, however, suppose all these conditions fulfilled, still it must be further asked, if the barometer be comparable, are the observations equally so? At what hours and under what circum-

* "Observations Sédentaires."

stances are they made? Is the temperature of the mercury taken into account? How are the thermometers and hygrometers constructed, and how placed? And what is the system adopted for the reduction of observations to a mean? Such questions are indispensable, yet it is to be feared many observers could not answer them in a satisfactory manner. We have a great number of barometric means collected from numerous observations, and yet no one can say precisely what these means are.

It is thus that many long series of observations are in reality lost to science, and only furnish illusory documents to the philosopher who wishes to draw inferences from the experiments of his predecessors. Let it be our endeavour that these losses shall be the last science has to sustain, and let us furnish our successors with points of comparison less equivocal.

Choice of Instruments.

The siphon barometer is preferable for stationary observations, as it possesses the peculiar property of annulling by compensation the effects of capillarity.

With cistern barometers the correction for capillarity must be applied as given in the table. But besides this, these barometers require the application of some means for reducing the level of the mercury in the cistern to the zero of the scale, a point from which it is continually deviating, as well by the ascent and descent of the mercury, as by the less apparent, though not less real, effect of changes of temperature. This is provided for by giving the cistern such a diameter that the variations of level become nearly insensible. This, however, is not sufficient for very exact observations.

It is almost superfluous to remark the necessity of rigid accuracy in the division of the scale. In barometers mounted in wood, the scale is generally marked on a plate of metal attached to the mounting. This will not satisfy those who are desirous of great accuracy. Heat, cold, moisture, dryness, affect wood in every direction, sometimes increasing and sometimes diminishing the distance of the scale from the point where its divisions are supposed to commence. The scale, therefore, should be entire from zero up to the point of the greatest elevation of the mercury, though it may be divided only in that part through which the range of variation extends. Copper is the best material as being the metal with whose pyrometrical dilatation we are best acquainted. With a scale thus constructed, we know exactly to what the variations in dimension which result from variations in temperature are reducible. They are regular and very small, and may generally be neglected; but we thus know their amount, and can supply the correction.

The vernier should indicate the 1-1000th of an inch; and this is nearly the utmost which the barometer is capable of expressing without ambiguity; the visual ray must of course be perpen-

dicular to the axis of the column. Some observers have added a microscope to discern more exactly the point of contact. This appears unnecessary, as we can judge without a microscope very well to the 1-1000th of an inch; and the adherence of the mercury to the sides of the tube is sufficient to destroy the exactness of even this observation; and in the most calm weather, the column is never perfectly at rest, but shows small though sensible oscillations. The approach of the observer, and the handling of the instrument, communicate to some of its parts a heat which has not time to distribute itself equally to the other parts, and the error resulting is generally something, if not equal to the 1-1000th of an inch: in fact, attempts at excessive accuracy in the observation have in general only the effect of making a display of figures in the result, which the real capability of the instrument will not warrant.

We ought surely to take the different means of ameliorating observations in the order of their importance; and no one who has ever employed the barometer with attention is ignorant that of all the errors imputable to the instrument, the most frequent and the most considerable are those which arise from a false indication of the temperature of the mercury. Now we do not here discuss minute fractions of the smallest divisions; one degree of the centigrade thermometer corresponds, on the barometric scale, with more than 3-100ths of an inch; and in elevation, with a difference of more than 3 feet. While we are at one end of the mercurial column attempting to estimate microscopic intervals, we must not forget that at the other, a slight and often inevitable mistake may make the observation lose in exactness ten times what it gains by the accuracy of the vernier.

The greatest improvement which could be introduced, and which would confer the greatest honour on artists, would consist in finding a method of uniting the correctional thermometer to the column of mercury in so close and immediate a manner, that its indications should, at all times, and under all circumstances, be the exact measure of the temperature of the column.

The precautions have hitherto been confined to inclosing the bulb of the thermometer in the mounting of the instrument, so that the variations of external temperature may be considered as affecting it only through the intervention of the mounting. This does not dispense with the observer's having an eye upon all that may disturb the accordance of the two instruments. Rapid changes of temperature are above all to be suspected, for the correctional thermometer always marks them before the entire mass of the instrument partakes in them; and we can do nothing better than to shelter the barometer as far as we can from the influence of these changes.

The barometer then must be furnished with a good thermometer well joined to its mounting, and we must never fail to combine its indications with those of the barometer. The extent

of the corrections which the heights of the mercury must undergo on account of the changes in the temperature of the instrument, sufficiently remind us that we must employ none but the best thermometers. The same remark will apply to the thermometers employed for the temperature of the air; but thermometers perfectly to be depended upon are, perhaps, of all the apparatus, the most difficult to procure. When the observer does not construct them himself, he cannot be too careful in employing none but the best artists; and even then it is not prudent to take their instruments without examination. Often the fixed points have been inaccurately determined; if these are exact, the calibre of the tube is often unequal, and equal degrees do not correspond with equal dilatations. In some instruments, this defect in the tube is to a certain extent corrected by an inequality in the divisions. These compensations, however, are not to be relied on, as being obtained generally from a very defective method; namely, the comparison of a small portable thermometer with a large standard one; but it is very difficult to be certain that the temperature communicated is, at the same time, exactly the same, in two thermometers, of very different volumes, and which gain and lose the same degree of heat in very different times. The artist desirous of making a really good instrument will never fail to verify the calibre of his tubes by the known methods, and absolutely to reject all those which do not stand test. But those comparisons just alluded to, which ought not to be employed for the *construction* of thermometers, may be employed to *try* them, at least approximately in the case of instruments where the mass is equal, and the structure similar. My mode of proceeding is this: I unite two or three together, the thermometers most similar in figure and dimensions, and subject them together to the heat of boiling water in a vessel placed on a chafing dish. This point, as is well known, is only fixed in respect to a certain pressure of the atmosphere; and in order to avoid having to make a reduction, it is convenient to make the trial under the standard pressure; that is to say, when the barometer is at 29·921 inches. This first test will infallibly detect the presence of small air bubbles otherwise imperceptible, and which interrupt in whole or in part the continuity of the thread of mercury. The heat dilates them, and we are at once warned of their existence by the rapidity with which the mercury rises above the boiling point. These bubbles altogether imperceptible ordinarily conceal themselves about the neck of the bulb, and this is a common fault in thermometers whose tubes are contracted in that part. Such must be rejected as the evil is irremediable. The point of ebullition being verified, I leave the apparatus to a well-regulated cooling process, and follow with my eye the progress of the thermometers. This trial would be very defective if the bulbs were of very different capacities, or if the heat diminished with too great rapidity; but

it possesses sufficient exactness with the precautions which I adopt. I arrive at length at the point of congelation, which, of the two fixed points, is that which has the greatest influence on that part of the scale which concerns meteorological observations. It will be readily supposed that two thermometers may be considered as being perfect in their calibres, if they go through this trial without disagreeing.

It can scarcely be necessary to mention that mercurial thermometers alone ought to be employed; and that in those used for taking the temperature of the air, the bulb ought to be entirely separate from the mounting. Small ones are preferable to large, as being more sensible and easier to use. It will suffice that the scale be large enough to subdivide the centigrade degrees by estimation into tenths. A greater precision would be superfluous, for the temperature of the air is rarely sufficiently constant for the uncertainty of the observation not to exceed a tenth of a degree.

Situation of the Instruments, and Method of observing.

The barometer ought to be in a perfectly vertical position; if it be not constructed in such a way as to take this position of itself, means must be used to place, and keep it so.

It is proper to keep it in a close place, the temperature of which varies but little, or changes very slowly. We shall thus be the more sure that the correctional thermometer expresses faithfully the temperature of the instrument. In order to profit by this accordance, the correctional thermometer should be observed before the barometer. Since the approach of the observer may modify the superficial temperature, and act on the thermometer before the variation has had time to be propagated to the tube of the barometer, which resists the communication as well by its mounting as by its volume.

In general, a northern or easterly aspect is preferable to that of the west or south. The impetuous winds which blow from these latter quarters occasion, by impinging on the walls, compressions of the air, which the mercury indicates by oscillations often very considerable, and always very inconvenient; the same thing will also happen if the walls or roofs opposite to the place of the barometer reflect, or disturb in different directions the currents of air which strike against them; and I have seen in such positions the column of mercury not only oscillating so as to render observation impossible, but sustained for whole hours above or below the point at which it stood in moments of calm. We must be guided by the local circumstances; we have only to choose for the barometer the situation where these causes will affect it least.

As to the thermometer, it ought to be freely exposed to the air, but should never be in the sun. In this last respect a northern exposure is the only one which is suited to it. But we must

also take care that it be beyond the reach of heat reflected by the ground, by walls, and roofs opposite. In our houses, we cannot place it too high: it does not do well except in the upper stories; and we may very well fix it on the sash frame, the shutter, or side of a window. The air ought to circulate freely round it. I usually suspend it by a hook, the arm of which is about six inches long. A ring fixed at the end of another arm of the same length embraces the instrument at its lower end, and secures it from being moved by the wind. I fix this small apparatus on the outside of a window upon the sash frame, in such a manner that the thermometer may be easily observed without opening the window.

But in thus exposing the thermometer to a free circulation of air, we must at the same time take care to defend it from the immediate contact of snow, hail, or rain; as often as it is touched by any of these, it is no longer the temperature of the air, but that of the meteor in question which it indicates. This object is attained if the roof have a sufficient projection. I prefer, however, a small moveable pent-house, placed at a convenient height, and which we may let down only when requisite; except in these cases, a shelter is rather pernicious than useful. In those winter nights, for example, when the calm of the atmosphere, the serenity of the sky, and the twinkling of the stars, announce a sharp frost, the thermometer will not indicate the whole intensity of the cold if a shelter be interposed between it and the particles of air, which, after being condensed in the middle regions, fall vertically upon the earth in an invisible shower. It ought in this case to be uncovered for the same reason that we cover an espalier which we wish to preserve from the frost.

When once the thermometer is well placed, the observation of it does not present any difficulty. The only attention which it requires is that of holding the eye exactly on the level of the point observed; for if we raise or depress it, if the visual ray deviate from the perpendicular to the axis of the tube, the surface of the mercury will successively correspond to different divisions of the scale. It will appear lower if we look at it from a higher situation; and higher if from a lower; and the error will be proportional to the angle which the visual ray forms with the perpendicular: this angle is what is termed the parallax. In observing the barometer, this source of error is annulled by means of the index or ring which fixes the line of sight. This method cannot be applied to the thermometer, which must be observed at a distance, and never handled. Attention will supply the want of it, and gradually become a habit.

It is useless to take greater precautions than these. The temperature of the air is often so inconstant, and undergoes so many alterations in the places in which we are often obliged to make our observations, that it would be in vain to seek in the

instrument for an exactness of which the observation itself is not susceptible. The uncertainty is in the thing itself, not in the indication of it. You have perhaps just observed the thermometer, and noted its indication: observe it again; it has varied: observe it again, it rises; it falls; and however small the range of these variations may be, that which was at first considered certain has become in a great measure problematical: you know not what to think for certain of the temperature of the air. There are some cases, however, where the choice is pointed out, whether by the nature of the place where we observe, or by that of the circumstances which manifestly act upon the thermometer. If it rise during short intervals of sunshine, we may lay this to the charge of reverberation. If it sink during gusts of mist, it is the temperature of the meteor which produces the change; but frequently also the change is owing to causes of a more general nature; as to the encounter of two currents of air of different temperature. To attain the greatest exactness we can, we should prolong the observation for some minutes in order to judge of the changes to which the instrument may be exposed; to inquire into the cause of its apparently capricious movements; and in a case of uncertainty, to take the mean between its extreme variations.

System of Observations.

To find the real mean barometric pressure is a matter of more difficulty than is commonly imagined. The mercury has in fact two species of oscillations, essentially different, although frequently confounded, from the effects of the agitations of the atmosphere. One sort is periodical and regular, the other, accidental and irregular. The idea of a barometrical mean necessarily imports that of a complete compensation between the one and the other.

If there were none but periodic variations, the mode of proceeding would be equally expeditious and simple. We should in this case soon settle the length of the periods, and the epochs of their recurrence. We should merely have to observe the barometer for some days at the particular hour of these periods, and to take a mean between the different heights of the mercury. This is actually the case within the tropics. There the accidental variations are almost reduced to nothing, and the periodical are very evident and regular. The barometer is at its maximum at 9, a. m. and 11, p. m.: at its minimum at 4, p. m.; and at 4½, a. m. this variation is constant: it is uniformly repeated every day: the succession of the seasons produces no alteration: the elements on which the mean is founded are, therefore, simple, distinct, and free from all mixture of interfering causes; and M. de Humboldt, who has founded the system of his observations on the phænomenon of the daily variation, has given us a barometric mean completely unequivocal.

In the temperate regions, the case is widely different—the frequency and extent of the accidental variations, disturb and disguise the effects of the diurnal. It nevertheless does exist, and attentive observation may soon detect it. We must, therefore, take it into account, and when it becomes a question to determine the mean pressure of the atmosphere, we must no more neglect the horary variations in the compensation between the accidental, than the accidental in the compensation between the horary. This certainly makes the problem a little more complicated. The difficulties increase, the task becomes longer; but the method of proceeding is not altered in its nature.

It is necessary, therefore, that in each series the observations should belong to the same hour; for every hour having its particular variation, a series composed of observations made at different times contains the diurnal variation as an indeterminate quantity, and under an irreducible form. In the next place, the hour of each series must coincide with one of epochs of the diurnal variation; for the comparison of series which belong to intermediate hours does but imperfectly compensate the deviations of the diurnal oscillation.

I have determined for our climates the progress of these horary variations. In summer the maximum is at 8, a. m. and 10 p. m.: the minimum at 4, a. m. and 4, p. m. In winter there is an hour's retardation in one of the four epochs, and an hour's advance in the others; they are respectively 3 and 9, a. m. and 3 and 9, p. m. In the spring and autumn, $3\frac{1}{2}$ and $8\frac{1}{2}$, a. m.; $3\frac{1}{2}$ and $9\frac{1}{2}$, p. m. We have then only to make four observations in the day at these four epochs; and to continue the series for a sufficient length of time to compensate accidental variations, and then to take the mean of each series, from which again we may take the mean for the day. But it may be asked, what must be the length of time necessary to obtain a compensation of the effects of accidental variations? To judge from the general practice we might suppose that the series must be continued for a period altogether indefinite; but in fact, however capricious the phenomena of accidental variations may seem, they yet recognize certain laws. Each epoch has its peculiar share of these variations, the result of which, after every compensation has been made, constitutes its character. This is the case with each different season. The system of observation must proceed by years; because the result of the year compensates the characteristic accidental variations of the different seasons; and a barometric mean ought not to include fractions of years, because it will then incline to the side of that season which is doubly represented. Equal probability can only be attained in periods of the same nature and extent. The mean of a complete year is only to be corrected by the mean of another year; and each enters into the common result for the half of the difference; a third year, for a third, &c. and as the differences are small in

respect to the extent and nature of the period, we shall soon attain the epoch where the correction is almost nothing, and the mean does not sensibly differ from being stationary.

In general the result of a year may be regarded as quite a sufficient approximation, and when a barometric mean is founded on two or three years' observations, we shall not risk much in regarding it as decisive; but if it be intended to serve for determining the relative or absolute elevation of the place, it is further necessary that it should be accompanied by a thermometric mean deduced according to the same method by observations in conjunction with those of the barometer. The methods most commonly pursued in conducting a series of meteorological observations are very far from tending to the attainment of the objects just specified. The observation of the maximum and minimum temperature of the day is of little use, and forms a part of a series altogether differing from that of the pressure.

We shall greatly simplify our labour if we can determine the instant when the height of the barometer is exactly the mean of its heights which correspond to the four epochs. Now this is not very difficult; it will readily be seen that this period will be found at a distance from the maximum and minimum determined by the ratio which exists between the oscillations of the day and those of the night. I find that in our temperate climates the hour of noon satisfies very well these conditions, and the coincidence is the more fortunate as the same hour is convenient for several other objects. If there be any error in the barometric mean of noon, it will probably be in excess, but extremely small in regard to the nature of the operation. I do not believe that it exceeds ·003 of an inch, a quantity which we may assuredly neglect on this occasion; with deference to the more rigorous determination and introduction of it into calculation, if ever the exactness of observations and observers should be carried so far that a barometric mean should correspond with the atmospheric pressure to the ·003 of an inch.

I make then no difficulty in regarding the mean of noon as a sufficient expression for the mean atmospheric pressure corrected for the diurnal variation; and for many years my system of observation has been founded on this basis. Observations made at the critical periods of the diurnal variation furnish the most certain presages which can be drawn from observation, of the progress of the barometer. It is by the derangement of the horary oscillations that the smallest changes introduced into the constitution of the atmosphere announce themselves. Lastly, the extent of the variation established in different places conjointly with the mean height of the mercury will establish points of comparison, by the help of which we shall form a more solid judgment on the ratio of the pressure of the air to its weight; a ratio which offers one of the newest questions in meteorology, and one which is the most fruitful in important consequences.

An attention to the foregoing considerations in conducting a series of meteorological observations will be accompanied by the following advantages.

1. The mean height of the barometer at noon, at the same time that it has the property of expressing the mean pressure of the atmosphere, disengaged from the diurnal variation, possesses also exclusively among all other means, the qualities required for the determination of differences of elevation. The coefficient of a barometric formula can never be exact but in reference to a fixed hour. Now the coefficient of M. de Laplace's formula is appropriated precisely to the hour of noon: it is a truly fortunate coincidence that we thus are enabled to determine the elevation of places, by the use of the same barometric means which have served to determine the respective pressures.

2. The morning, afternoon, and evening observations made at the critical hours of the daily variation, after having been of daily utility for foreseeing changes of weather, have besides the advantage of fixing, for each climate, the extent and circumstances of the variation: and each series separately reduced to its mean expression, being employed in the calculation of differences of level, instead of the mean of noon, will give the measure of the error arising from the hour; and consequently the correction which the coefficient requires in order to become applicable to that hour.

I will conclude with one consideration of which we must never lose sight. Barometric means cannot be employed to determine the elevation of distant places above one another, except so far as their respective climates continue the same. The climate has a powerful influence on the variable ratio which subsists between the weight and the pressure of the column of air. These two quantities, perhaps, attain equality at the mean parallel where all meteorological influences are in a state of equilibrium; and in this case it will be true for us, that the mean height of the mercury expresses exactly the mean weight of the atmosphere; but at the same time that we find the temperate regions enjoying this advantage, it also follows that no others partake of it. The pressure diminishes in proportion as we approach the equator; and on the shores of the south sea, the barometer stands lower than on our western coasts. This same pressure increases as we approach the pole, and the barometer, *cæteris paribus*, would stand higher on the shores of the arctic sea than in our latitudes: even between the northern and southern parts of France, the differences may become sensible; and though Geneva is not far from the Mediterranean, the difference of climate is such that the absolute elevation of its lake would be but ill established if it rested only on the comparison with observations made at Marseilles.

Observations * *for the Measurement of Heights.*

In addition to what has been before said of the instruments, and of the manner of using them in general, the following remarks apply to the observations on mountains.

The mountain barometer should be of such a construction as to be neither liable to be easily broken, nor to the introduction of air bubbles. It should be easy to try whether the instrument takes the vertical position well, and continues in it; and I regard as a peculiar merit, such a construction that it rapidly acquires the temperature of the place. The resistance of the mounting to variations of temperature exposes the observation to inaccuracies, causes loss of time, and occasions many errors.

Observations for the measurement of heights necessarily suppose corresponding observations if any exactness be intended; and the two barometers ought to be perfectly comparable, or the precaution will be in vain. In this case, the mere presumption that they agree is not sufficient; we must carefully compare the instruments, and if the operation be delicate, and we wish for great precision, it is not enough to have made the comparison beforehand alone, but it is prudent to do so after the operation also; for the portable barometer may have undergone some derangement in carrying. When the two instruments do not sustain the mercury exactly at the same point, if the difference is not great, and especially if it is such as may be owing to the difference of the diameters of the tubes, there is no reason for suspecting either of them; and we may be content to allow for the difference in the calculation. In the case where either of the two instruments does not sustain the mercury at its absolute height (which cannot fail to happen if they are both of the cistern construction), it will be proper to correct them for the effect of capillarity: for the depression resulting from this cause is sometimes sufficient to introduce a sensible error into the measurements.

One great difficulty consists in finding a suitable place for the portable barometer: it is necessary to preserve it from rapid changes of temperature; yet it is almost always exposed to the free air where the temperature is continually varying. The instrument ought to be kept in the shade; yet it is very often exposed to the sun, which acts very unequally on its different parts, whether by direct or reflected rays. The consequences of such a position are more easy to conceive than to avoid: in the sun, on the one hand, the tube becomes heated; the cistern again is further heated by the reverberation of the ground: the correctional thermometer indicates a temperature more or less elevated according to the direction in which we turn it: then

* "Observations Ambulantes."

perhaps come currents of air which modify the causes of error; an interval of calm restores to them their energy; the intervention of clouds instantly suspends their action: in the midst of such a complication of effects nothing seems clear except motives for doubt; and the observer is neither unsuccessful nor unskilful if he knows, within one or two degrees, the mean temperature of his instrument.

I mention these inconveniences, because it is necessary to have a just idea of them in order to be in a situation to obviate them according to the exigency of the case, and the means which the situation or chance place at our disposal, when foresight has not been able to provide against them. A rock or a tree frequently afford a convenient shelter. We may supply the want of them, at least in part by a man placed between the sun and the instrument: by a piece of linen fastened round the tripod which supports the barometer; or for want of any other resource, by making the shadow of one of the legs fall along the tube, and especially over the cistern. The thermometer ought always to be turned away from the sun. When the alternations of wind and calm cause variations too sudden and too great, I cover the bulb in such a way as to defend it to a certain extent from these passing and capricious sources of variation. The action which they exert on the thermometer will induce an error upon the temperature of the instrument, because these very transient variations may have time to make an impression on the surface of the mounting, but not to be communicated to the entire mass of mercury.

With respect to the thermometer employed to mark the temperature of the air, it is always in the most elevated, the most exposed, the most airy situation, that its place ought to be chosen. This condition is much more easily fulfilled in an open country, and on the summit of a mountain exposed to all winds, than in buildings where we commonly make meteorological observations. There our stationary thermometers have communication only with half the surrounding atmosphere: the other half is kept from them by the wall against which they are placed; but this disadvantage is compensated by the facility of placing them at an elevation where they are secure from the effects of reverberation from the earth: this resource is wanting in mountain observations. We cannot place the thermometer higher than the point where we can observe it without parallax; and at this elevation, which does not exceed that of the human body, the instrument is far from being out of the reach of the earth's influence; this inconvenience is unavoidable: we ought, therefore, to lose none of the advantages which accompany it. It is not without reason that Saussure condemns the practice of suspending the thermometer from a body of any magnitude. He attaches it to a simple staff, the shadow of which, directed upon the bulb, is sufficient to shade it from the sun, and the diameter

of which is too small either to communicate its heat to it, or to create an obstacle to the free circulation of the air, when the thermometer is fixed at a due distance from it. I have always followed the same plan. A staff of about six feet in length answers the purpose; it has an iron point to fix it in the ground. At the other end are two holes to receive two small arms of iron or brass, about five inches long; one terminated by a hook, the other by a ring, the end of each which is to fit into the hole is formed into a screw. The arm with the hook is fixed in the upper hole, and serves to suspend the thermometer; that with the ring in the lower, and retains it in a position parallel with that of the staff. The staff serves for myself or my guide, and the arms are carried in the bag which holds the thermometers. I do not believe it possible to attain the requisite precision in the indications with less apparatus.

The observation of the thermometers is the most delicate and the most difficult part of the operations; and most of the faults which we commit in the measurement of heights may be traced to a false valuation of the temperature of the air, or of that of mercury. I have mentioned this before, but there is no harm in repeating it, and we cannot be too careful in pointing out the sources of error, especially when they are of such a nature as easily to disguise themselves to inattentive eyes. The inexperienced observer, when he meets with unsatisfactory results, will be less tempted to lay the blame on his instruments, or on his formula; and this may often spare men of speculative minds the trouble of imagining new theories to correct in the formula, irregularities, which exist only in the observations.

All philosophers who have been engaged in barometrical measurements must doubtless have made the same remark which I have. They cannot have carried the thermometer to summits of mountains, exposed to all winds, without often experiencing the same embarrassment which I have. The thermometer has varied, with them as well as with me, every instant in proportion to the degree of wind, of calm, the presence of the sun, or the interposition of clouds. These variations they have not neglected, because they could not misinterpret them. Like me they have often been in uncertainty as to the real and intrinsic temperature of the air, and that of their instruments; and have not assuredly confined themselves to noting down at all hazard merely the temperature which any accident may cause to prevail at the exact moment of observation. But if they are generally silent on a point which cannot escape any experienced person, it is that they suppose the logic of the observations familiar to all those who employ meteorological instruments with any discernment. I conceive I ought not to imitate them in addressing myself to beginners. Such readers require advice and examples. I will give one or two, and I do not select the most rare cases.

Aug. 30, 1805.—On the summit of the Pic de Midi, between 10, a.m. and 1, p.m. the thermometer varied from 14° to 19°. This was owing to an irregular wind. In moments of calm, it stood at 16° or 17°, this being partly caused by heat of the surface: it fell when the wind brought against it masses of colder air; and rose to 18° or 19° when the current continued. In this complication of influences, the only way was to take a mean between the extremes; for it would have been equally absurd to keep to either 14°, 16°, or 19°, for the sole reason that the thermometer had accidentally indicated one of these degrees at the precise moment when we noted the height of the barometer.

Sept. 11.—On the Pic de Bergons, there was a brisk wind subsiding at intervals. The temperature of calm was above 14°, but the ground reflected much heat. It is seldom that this is not the case with the observed temperature of a calm air. This temperature, however, increased as the wind rose: it was a different atmosphere which the wind drove before it, heated by passing over the neighbouring plains. It rose to 15·5°, but when the wind continued constant, sunk to about 13°, and there became stationary, which was certainly the true temperature of the air under the prevailing modification, the higher temperatures were transient and accidental.

Aug. 10, 1802.—On the summit of Mont Perdu, a thermometer, placed on the snow, sunk to − 2·5°, owing to the rapid evaporation. Another suspended at the height of about 4½ feet partook of the same influence, and never rose above 4° or 5°. Another at the same time suspended above a rock free from snow stood at 12·5°, and one placed on the rock at 22·9°. The continuance of a wind (which at first brought hot air from the plains, gradually reduced them all to about 7·5°, nearly the mean; this was the true temperature of the air.

It is superfluous to mention a multitude of other cases differing but little from these; such as a passing shower which causes a variation in the thermometer the moment it touches it: a local fog, which occasions, in the particular atmosphere of the instruments, a cooling, in which the rest of the stratum of air does not partake: the influence of the sun which raises the thermometers; the intervention of clouds which makes them sink; all the variations which originate in reverberations or absorptions of heat, or in currents of air which are accidental and of limited influence; every thing which conspires to alter the general temperature of unfavourable situations, such as deep valleys, and even eminences above which greater heights immediately rise. I have said enough to awaken and direct the attention of those who wish, and know how, to be exact.

Such are the considerations which must guide us in reference to the thermometer for the air. We have just seen that the thermometer for the barometer is the object of considerations very different; for it, the temperature of the air is only a matter

of secondary importance. It is always well placed wherever the barometer is placed. Its variations are of no consequence, provided the temperature of the barometer undergoes the same changes; but this accordance is the thing of which we must be careful to assure ourselves, and this is the difficult point. It is very seldom that the two instruments, joined together as they usually are, will preserve this accordance when the temperature undergoes great and frequent variations. We may diminish the sensibility of the thermometer; but in thus preventing its indications from outstripping the rapidity of the changes which the temperature of the barometer undergoes, we must take care not to approach the point where, on the other hand, it may experience a retardation; and in all cases there is a wide field open to doubt and conjecture.

We should commence our operations by disposing the instruments in a convenient manner, and should then allow them time to lose the heat which they have contracted in carrying, and to acquire, each in its proper manner, the local temperature. This time is considerable for barometers cased in wood, which become unequally heated in the hands, or on the backs, of those who carry them. The heat thus acquired never distributes itself uniformly, and is dissipated with extreme slowness. Often an hour does not suffice to bring the barometer to an agreement with the thermometer, and with itself. More than a quarter of an hour is not necessary for barometers mounted in copper, and this is one reason for giving them the preference.

The time thus occupied, however, is not lost: we can make trial of the position: it may not always be suitable for the barometer: if it be too much exposed to wind, we can seek for a shelter. At the summit of a mountain, a strong wind has other inconveniences besides that of agitating the instrument: it rises on the acclivity against which it directs itself, and forms an ascending current, which bears up the column of air, and depresses that of the mercury. In such a situation the thermometer should be left; but the barometer should be removed from this part of the acclivity, and if the summit has not sufficient extent, we should leave it, and seek for a calm under the shelter of the opposite side, only being careful to allow for the quantity of our descent: then we shall await the moment of observation in considering the changes of the thermometers. We shall remark attentively how the one is affected in the free air, and the other in the mounting in which it is inclosed; whether they undergo considerable and frequent variations, or whether they reduce themselves by slow degrees to a stationary point. In this last case, we shall have no occasion to doubt the success of the operation: in the former, we shall examine the causes of the variations: we shall take an exact account of the nature and influence of every accidental circumstance. We shall repeat this examination after the observation in order to

state every thing which we have been able to ascertain with the greatest certainty during the short period of time of which it occupies the middle. The rule for the measurement of heights supposes that we know with great precision the temperature of the air, and that of the instrument: both these then we must discover, and when they disguise themselves, must divest them of their disguises. There are times when this is altogether impossible; but it is at least something to perceive this, and to know that we have to doubt an observation, well performed in itself, but of which we cannot be sure that we have entirely satisfied some of the fundamental conditions.

Under some circumstances, and those very common, the local temperature is so predominant, that in spite of all our care, it will enter all our estimates: it is in vain to guard against it. Measurements made in a hot season, and under a burning sun, always tend to err in excess; especially if the station be of such a nature as to multiply the reverberations of heat. On the other hand, they will err in defect in foggy or rainy weather, especially if the place is so circumstanced as to concentrate the cold. The error proceeds from the thermometers. To point out its origin is to warn the observer to avoid, if he can, the circumstances which produce it; and if he has not the choice, to allow for their influence in the opinion he forms of his measurements.

(To be continued.)

Article II.

A List of Substances arranged according to their Thermoelectric Relations, with a Description of Instruments for exhibiting Rotation by Thermoelectricity. By the Rev. J. Cumming, MA. FRS. and Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cambridge, July 23,* 1823.

The following tables will, I hope, be interesting to those who have read my communication to you in April last. The first contains the thermoelectric relations of different substances, with copper wires; the second, their relations to each other taken two and two together, each substance being positive to all below, and negative to all above. The voltaic series, and the order of conductors of electricity and heat, are added, merely to show that the thermoelectric series has no accordance with either of them.

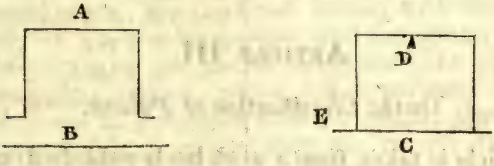
*Electromagnetic Relations of different Substances with Copper Wires; the Magnitude of the Substance examined being greater than that of the Copper Wire, excepting those marked *, none of which exceeded half a Grain in Weight.*

Postive Series.	Negative Series.
Bismuth,	*Iridium and osmium,
Mercury,	Rhodium,
Nickel,	Gold,
Platina,	Zinc,
Palladium,	Arsenic,
Cobalt,	Iron,
Silver,	Antimony,
Tin,	1 bismuth + 1 tin,
Lead,	1 zinc + 1 tin,
Copper,	1 zinc + 1 lead,
Brass,	4 zinc + 1 antimony,
Solder (common),	Printer's type,
Pewter,	Fusible metal,
*1 nickel + 1 iron,	1 ditto + 1 arsenic,
4 tin + 1 antimony,	*1 nickel + 1 palladium,
Galena.	*1 nickel + 2 platina,
	†1 bismuth + 1 zinc,
	Sulphuret of antimony,
	Charcoal (box wood),
	Plumbago.

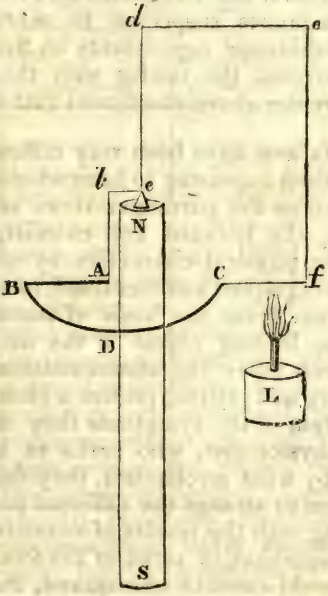
Thermo-electrics.	Voltaic series.	Conductors of Electricity.	Conductors of Heat.
Bismuth			
Mercury, Nickel	Charcoal	Silver	Silver
Platina	Platina	Copper	Gold
Palladium	Gold	Lead	Tin
Cobalt	Silver	Gold	Copper
Silver	Copper	Zinc	Platina
Tin	Lead	Tin	Iron
Lead	Tin	Platina	Lead
Rhodium	Iron	Palladium	
Brass	Zinc	Iron	
Copper			
Gold			
Zinc			
Charcoal, Plumbago			
Iron, Arsenic			
Antimony			

† The compound of bismuth and zinc, it is well known, is not an alloy, yet it acted negatively whether the heated part appeared to be zinc or bismuth. The compound ore of iridium and osmium was from Dr. Wollaston: the alloys of nickel from Dr. Clarke, formed by the gas blowpipe.

The combination of platina and iron is very powerful, and has the advantage of permitting the application of great heat. That of platina and silver is readily applied to exhibit the *inverse* experiment, i. e. the motion of the thermoelectrics on the approach of a magnet. A silver wire bent into the form A is connected with a platina wire B into the form C E D F, either by soldering or by fine platina wire; the whole is suspended from a point D. On heating one extremity E, and applying the pole of a magnet to F, the apparatus revolves from left to right, or *vice versâ,* according as the pole of the magnet is N or S. The apparatus I have used for the purpose weighs nine grains: indeed I know no limit to its minuteness.



The annexed figure represents an arrangement for producing a perpetual rotation, by means of platina and silver wires poised upon a magnet, and heated by a spirit-lamp. A B D C platina wire; A *b c d e f* C, silver wire; *c* N, support of the wires; N S, magnet; L, spirit-lamp.



The platina wire being considerably thicker than the silver, the part A B will balance the projecting part of the silver wire *d e f* C. A wire is attached to *d e* at right angles with a small weight to counterbalance B D C.

P. S. The electromagnetic multiplier mentioned in your number for June, is, I perceive, a similar instrument to that which I used and described as a Galvanoscope two years and a half since, in a paper published in our Cambridge Transactions, and with which all my present experiments were made.

Very sincerely yours,
J. Cumming.

Article III.

On the Classification of Poisons.

[This article is taken from a work lately published on Medical Jurisprudence, by J. A. Paris, MD. FRS. &c. and J. S. M. Fonblanque, Esq. Barrister at Law. It would not be consistent with our plan to enter minutely into an account of a work of this nature. It contains, however, so much curious matter connected with chemical science, on the subject of nuisances and poisons, that we intend, in a future number, to give from it, and other sources, a general and comprehensive view of the methods of examining substances suspected to contain poison, with observations and additional experiments on the subject. In the mean time, we present the reader with the classification of poisons adopted by the above-mentioned authors.—*Edit.*]

Poisonous substances have been very differently arranged by different authors, each appearing to have adopted a classification best suited to promote the particular views and objects of his own pursuit; thus, the botanist and chemist, engaged in the examination of the physical characters by which poisons may be individually distinguished and identified, have very judiciously erected their system upon the basis of natural history. The pathologist, whose leading object is the investigation of the morbid effects which follow the administration of these agents, with equal propriety and justice, prefers a classification deduced from a generalization of the symptoms they are found to occasion; while the physiologist, who seeks to ascertain through what organs, and by what mechanism, they destroy life, may be reasonably expected to arrange the different poisons under divisions corresponding with the results of so interesting an inquiry.

To meet the comprehensive views of the forensic toxicologist, an arrangement would seem to be required, that should at once embrace the several objects which we have just enumerated;

for the data from which the proof of poisoning is to be inferred, are, as we have often stated, highly complicated in their relations. No such classification, however, can be accomplished, and we are therefore compelled to select one which may approach the nearest to our imaginary fabric. That which was proposed by Fodéré, and adopted, with some trivial alteration in the order of succession of the classes, by Orfila, in his celebrated system of toxicology, although it has many defects and some errors, nevertheless merits the preference of the forensic physician; its basis is strictly pathological, and yet it distributes the different poisons, with some few and unimportant exceptions, in an order corresponding with that of their natural history.

The first two classes, for instance, present us with substances of a mineral origin; the third and fourth, with those which are principally of a vegetable nature; and the sixth, with objects chiefly belonging to the animal kingdom. The importance of acknowledging a division, which has a reference to the three great kingdoms of Nature, is perhaps greater than the reader may anticipate; for in enumerating the various experiments to be instituted for the detection of poisons, we are, by such an arrangement, enabled to bring together a connected series of processes, nearly allied to, intimately connected with, and in some respects, mutually dependant upon each other.

The following is the arrangement of Fodéré as modified by Orfila; viz. Cl. I. Corrosive, or Escharotic poisons. Cl. II. Astringent poisons. Cl. III. Acrid or Rubefacient poisons. Cl. IV. Narcotic or Stupefying poisons. Cl. V. Narotico-Acrid poisons. And Cl. VI. Septic or Putrefying poisons.

Class I. *Corrosive or Escharotic Poisons.*—Such as corrode and burn the textures to which they are applied. When internally administered, they give origin to the following symptoms: violent pain, accompanied with a sense of heat and burning in the stomach, and throughout the whole extent of the alimentary canal; frequent vomitings, often sanguineous, and alternating with bloody diarrhœa, with or without tenesmus; the pulse hard, small, frequent, and at length imperceptible; an icy coldness of the body; cold sweats; a great anxiety and oppression at the præcordia; and hiccup. Sometimes the heat of the skin is intense, the thirst inextinguishable, and the unhappy patient is tormented with Dysuria and Ischuria, violent cramps in the extremities, and horrid convulsions, which are relieved only by death. Such are the general symptoms by which this species of poisoning is characterised; the rapidity with which the symptoms terminate their course will depend upon the violence of the dose, and the particular species of poison which has produced them: there are, moreover, other symptoms which will be more conveniently described, when we come to speak of the effects of corrosive poisons individually. In this class are

ranked the following substances:—Metals. I. *Arsenic.* 1. Arsenious Acid, or White Oxide of Arsenic. 2. Arsenites, or Combinations of that Acid with salifiable Bases. 3. Arsenic Acid. 4. Arseniates, or Combinations of the preceding Acid with the Bases. 5. Sulphurets of Arsenic, or Orpiment and Realgar.—II. *Mercury.* 1. Corrosive Sublimate of Mercury, or Oxymuriate of Mercury. 2. Red Oxide of Mercury. 3. Red Precipitate, or Nitric Oxide of Mercury. 4. Other preparations of Mercury.—III. *Antimony.* 1. Tartarized Antimony, or Tartar Emetic. 2. Oxide of Antimony. 3. Antimonial Wine. 4. Muriate of Antimony, or Butter of Antimony.—IV. *Copper.* 1. Blue Vitriol, or Sulphate of Copper. 2. Verdegris. 3. Oxide of Copper. 4. Other preparations of Copper.—V. *Tin.* 1. Muriate of Tin.—VI. *Zinc.* 1. Sulphate of Zinc, or White Vitriol. 2. Oxide of Zinc.—VII. *Silver.* 1. Nitrate of Silver, or Lunar Caustic.—*The Concentrated Acids.* 1. Sulphuric. 2. Muriatic. 3. Nitric. 4. Phosphoric, &c.—*Hot Liquids.* 1. Boiling Water. 2. Melted Lead.—*The Caustic Alkalies.* 1. Potass. 2. Soda. 3. Ammonia.—*The Caustic Alkaline Earths.* 1. Lime. 2. Baryta. 3. Muriate and Carbonate of Baryta. *Cantharides. Phosphorus.*

Class II. *Astringent Poisons.*—They occasion a remarkable and unrelenting constriction of the great intestines, especially the colon, so as to resist the operation of the most powerful cathartic remedies. Violent cholics ensue, and partial paralysis; in the end if the dose be sufficiently large, or if small doses have been frequently repeated, they will excite inflammation of the alimentary canal, but it is not succeeded by that disorganization which generally characterises the operation of poisons belonging to the preceding division. We rank under the present class only the preparations of lead, viz. 1. Acetate of Lead, or Sugar of Lead; 2. Oxides of Lead, Red Lead, Litharge; 3. Various saturnine impregnations.

Class III. *Acrid or Rubefacient Poisons.*—These poisons are known by their producing an acrid taste, more or less pungent and bitter; a burning heat, and considerable dryness in the mouth and fauces; and a constriction, more or less painful, in the throat. Acute pains are, after a short interval, experienced in the stomach and bowels, which are quickly followed by copious vomiting and purging, and which continue, with the most painful efforts, long after the alimentary canal has been completely evacuated. A few hours after, phenomena are observed, which indicate a lesion of the nervous system, such as vertigo, dilated pupils, dejection, insensibility, laborious respiration, and death. The lesions of texture, occasioned by the action of acrid poisons, have the greatest analogy to those produced by corrosive poisons; in fact, says M. Orfila, we do not hesitate to declare, that there exists a perfect identity between

the alterations of the digestive canal produced by the poisons of these two classes, when introduced into the stomach." The substances included under this class belong, for the most part, to the vegetable kingdom, such as scammony, camboge, black and white hellebore, bryony, euphorbium, seeds of the ricinus, iatropa curcas (Indian nut), croton tiglium, squill, aconite, &c. &c.

Class IV. *Narcotic or Stupefying Poisons.*—Such as occasion stupor, drowsiness, paralysis, or apoplexy, and convulsions. They do not produce any change in the structure of parts to which they are applied. M. Orfila has satisfactorily ascertained that no alteration can be discovered on dissection in the digestive canal of persons who have swallowed any one of the poisonous substances of this class.

Class V. *Narcotico-Acrid Poisons.*—This division, as its name implies, is intended to receive such substances as produce the united effects of those belonging to the two preceding classes, acting for instance at the same time, as narcotics and rubefacients. Amongst the articles of this class, the following may be enumerated, Belladonna, stramonium, tobacco, foxglove, hemlock, nux vomica, camphor, cocculus indicus, certain mushrooms, alcohol, &c. &c.

Class VI. *Septic and Putrefying Poisons.*—By this term are included those poisons which, according to Orfila, "occasion a general debility, dissolution of the humours, and syncope, but which do not, in general, alter the intellectual faculties." The articles of this class belong almost entirely to the animal kingdom, with the exception perhaps of a few gaseous compounds, and the spurred rye, or ergot, viz. *venomous animals; animals whose fluids have been depraved by antecedent disease; the poison of fishes; substances in a state of putridity; spurred rye, or ergot.*

Such is the classification which, for reasons already stated, it is our intention to adopt on the present occasion. We shall, however, in an additional chapter, under the title of "Aërial Poisons," treat of those substances which are exclusively capable of acting upon the body through the medium of the atmosphere, or which require to be in a state of vapour, or gas, to ensure their operation.

With regard to the classification of Fodéré and Orfila, we must here observe, that we follow it only conventionally, and that while we acknowledge it as being very convenient for the consideration of poisons, in reference to their forensic relations, yet we must not be considered as insensible to its many defects and fallacies. In the first place, it has little or no reference to the enlarged views of the modern physiologist, respecting the "*modus operandi*" of poisons; nor indeed is its construction susceptible of such modifications and improvements, as can

ever render its degree of perfection progressive with the advancement of science. In the next place, the classes are in many particulars ill-defined, and indistinctly, if not erroneously, divided. How questionable, for instance, are the boundaries which separate corrosive from acrid poisons? even the respective species of each class are, in many instances, less allied to each other than the great divisions to which they are subordinate. As an exemplification of this fact we have only to compare the physiological actions of arsenic and corrosive sublimate; the former of these substances occasions death by being absorbed, and thus acting as a vital agent, the latter, by its local action as a caustic on the textures with which it comes in contact. In the same manner, if we examine the individual actions of the different species composing the class of "Acrid" poisons, we shall find the same want of uniformity; thus the spurge-flax, and the jatropa curcas, act by occasioning a local inflammation, while the hellebore, being rapidly absorbed, exerts a fatal action on the nervous system, and produces only a very slight inflammation. The class of narcotic poisons is more absolute in its definition, and more uniform in its physiological affinities, and therefore less objectionable, than the divisions to which we have just alluded; but the propriety of the term "Narcotico-Acrid" may be very reasonably questioned; even Orfila expresses his doubts upon the subject, "because the narcotic or sedative effects only follow the previous excitement." Some of the poisons, under this last mentioned class, are rapidly absorbed, and act, through the medium of the circulation, on the nervous system, without producing any local inflammation; whilst others, again, merely act upon the extremities of the nerves, with which they come in contact, and without being absorbed, occasion death by a species of sympathetic action.

These few objections, and many more might be adduced, are sufficient to demonstrate the imperfection of the classification under consideration, and which would render it wholly unavailable to the pathologist who must adopt his treatment according to the physiological action of each poison. The author has accordingly, in his "Pharmacologia," ventured to propose an arrangement, in conformity with such views; and the following sketch of it may perhaps form a useful introduction to the general observations which it will be hereafter necessary to offer upon the "*modus operandi*" of poisons.

A Classification of the different Modes by which Poisons produce their Effects.

I. *By acting through the Medium of the Nerves, without being absorbed, and without exciting any local Inflammation.*

a. By which the functions of the nervous system are destroyed.

Acrid.	Narcotico-Acrid.	Narcotic.
Aconite.	Alcohol.	Essential Oil of Almonds.†
Jatropa Curcas.	Oil of Tobacco.	Camphor.†
		Opium † ?

b. By rendering the heart insensible to the stimulus of the blood.

Infusion of Tobacco.

Upas Antiar.

II. *By entering the Circulation, and acting through that Medium with different Degrees of Force, on the Heart, Brain, and Alimentary Canal.*

Corrosive.	Acrid.
Arsenic.	Hellebore.
Emetic Tartar.	Savine.
Muriate of Baryta.	Meadow Saffron.
	Squill.

Narcotic.	Narcotico-Acrid.
Opium.‡	Deadly Nightshade.‡
Lettuce.	Hemlock.
Henbane.	Camphor.‡
Prussic Acid.	Cocculus Indicus.

III. *By a local Action on the Mucous Membrane of the Stomach, exciting a high Degree of Inflammation.*

	Acrid.
Corrosive Sublimate.†	Bryony.
Verdegris.	Elaterium.†
Muriate and	Colocynth.†
Oxide of Tin.	Camboge.
Sulphate of Zinc.	Euphorbium.
Nitrate of Silver.	Hedge Hyssop.
Acids.	Croton Tiglium.
Alkalies.	Ranunculi.
Cantharides.†	

† This mark denotes that the substance against which it is placed, may also act by being absorbed.

‡ Signifies that the article has also a local action.

The preceding classification of poisons will not only furnish the practitioner with a general theorem for the administration of antidotes, but it will suggest the different modes and forms of administration of which each particular substance is susceptible; it will show that certain poisons may occasion death without coming into contact with any part of the alimentary canal, and that others will produce little or no effect, however extensively they may be applied to an external surface. The first class comprehends such poisons as operate, through the medium of the nerves, upon the organs immediately subservient to life; in the application of such agents it is obvious that they cannot require to be introduced into the stomach, they may convey their destructive influence by an application to any part duly supplied with nerves, and whose extremities are exposed to their action; although at the same time, it may be observed that, in general, poisons of this kind act most powerfully when internally administered, owing to the extensive sympathetic relations of this central organ over every function of the living body. The second class consists of poisons that are incapable of producing any effect, except through the medium of the circulation; whence we shall be enabled to explain and appreciate the various circumstances which may accelerate or retard their operation. Poisons of this class may be applied externally to abraded parts, or even to surfaces covered with cuticle, provided their absorption be promoted by friction; and it may be here observed, that the function of absorption is not performed with the same force in every tissue; as a general proposition, it may be said to be energetic in proportion to the number of lymphatics and veins, although the late experiments of M. Majendie have shown how greatly it is influenced by the state of the circulation. If these poisons be administered internally, they find their way into the circulating current either through the branches of the thoracic duct, or those of the *vena portarum;* when, as if by a species of election, each substance very frequently expends its venom upon some one particular system of organs. Many of the substances arranged under this second division have moreover a local effect upon the structure with which they first come in contact; it is thus with colocynth, and some other bodies; while, on the contrary, several of those poisons which are distinguished for their *local action*, are subsequently absorbed, and are thus, as it were, enabled to ensure their work of destruction by a double mode of operation. We shall receive ample evidence of this truth, as we proceed in the history of particular poisons. The third class comprises such agents as inflict their vengeance upon the mucous membrane of the stomach, by actual contact, and destroy, by exciting local inflammation.

Article IV.

Analysis of James's Powder. By Richard Phillips, FRS. L. & E.

In the *Annals* for October, 1822 (New Series), I gave an analysis of the pulvis antimonialis of the London Pharmacopœia, by which it appeared that this medicine, procured from two respectable sources, differed very little in composition. The mean of the two results showed this preparation to be composed of

Peroxide of antimony.	36·5
Phosphate of lime.	63·5
	100·0

Several medical friends have since inquired of me, whether I had made any experiments upon James's powder, which has been shown by Dr. Pearson to consist of oxide of antimony and phosphate of lime. In answer to these inquiries, it might have appeared sufficient to refer to Dr. Pearson's analysis, published in the Philosophical Transactions for 1791. In the course of 30 years, however, chemical research has been so actively pursued, that it would be very remarkable if the nature of the oxides of antimony was not better understood than when Dr. Pearson performed his analysis. Without, however, entertaining any suspicions that the results of his investigation were incorrect, it appeared to me to be a subject worthy of further inquiry, whether the antimony in this powder is in the same state as I found it to be in the antimonial powder. The nature of the oxide formed no part of Dr. Pearson's inquiry, nor was the difference of power between the protoxide and peroxide of antimony so well understood as at present.

Having procured some James's powder,* I first directed my attention to the effect produced by boiling it in distilled water; my reason for this was to ascertain whether it contained any tartarized antimony, a suspicion of which was entertained by the late Dr. George Fordyce.

The water in which the powder had been long boiled was extremely turbid, and remained so for a long time; nor could I render it perfectly clear even by filtering it through several folds of paper.

On adding some solution of sulphuretted hydrogen to the filtered but slightly turbid water, perceptible traces of the presence of oxide of antimony were indicated by the appearance of the well-known orange-coloured precipitate; the effect was, how-

* From Messrs. Newberry's, St. Paul's Church-yard.

ever, so very trifling, that I am induced to attribute it entirely to the oxide of antimony suspended in the water, and not to any which it held in solution. To another portion of the solution, I added nitrate of lead; it was rendered rather more turbid by this addition, but not in a greater degree, than might be expected to arise from the presence of phosphate of lime, which is sufficiently soluble in water to be detected by reagents. Had tartarized antimony been present in such quantity as to influence the nature of the preparation, a copious precipitation of tartrate of lead must have occurred, instead of the slight turbidness described. Judging from the effects which I have now detailed, I am certainly of opinion that James's powder does not contain any tartarized antimony, or any combination of it which is soluble in water.

Fifty grains of James's powder were now boiled in an ounce of muriatic acid diluted with an equal bulk of water, and the ebullition was continued long after any of the powder appeared to be dissolved by the acid. It was evident that a very large proportion of the powder subjected to experiment remained undissolved. This circumstance perfectly satisfied me, that although James's powder might possibly contain some protoxide of antimony, a very large portion was evidently peroxide. I may here remark, that the degree of insolubility of this or any powder containing oxide of antimony, furnishes a ready mode of estimating its power; the less which is left undissolved, the more active the remedy; for, excepting under peculiar circumstances, peroxide of antimony is nearly insoluble in muriatic acid; and when it has been once subjected to a red heat, a very small quantity escapes such a degree of cohesion as to remain soluble in an acid.

Having suffered the muriatic solution to become clear by subsidence, I poured some of it into a large quantity of water; not the slightest precipitation occurred: it was, therefore, evident that but little, if any, oxide of antimony had been taken up by the muriatic acid.

As the excess of muriatic acid employed was considerable, I thought it might possibly retain the oxide in solution even after dilution with water. To discover whether this was the case, I added carbonate of soda to the muriatic solution until precipitation commenced. I then poured it into a solution of potash, taking care to have such an excess of the alkali as would immediately redissolve any oxide of antimony which might be at first precipitated.

In order to be certain that any oxide of antimony which the muriatic acid had dissolved should be taken up by the potash, the alkaline solution, containing the precipitated phosphate of lime, was boiled for a considerable time; the clear solution was poured off, and saturated with acetic acid, by which a very small quantity of precipitate was obtained. When this had been

washed, solution of sulphuretted hydrogen was added to it, and gave slight traces of the presence of oxide of antimony: it was, however, so evidently mixed with some impurity that the colour was reddish-brown instead of bright-orange: the quantity was also so extremely small that it would have been nearly impossible to have ascertained its weight.

I may here observe, that it is scarcely requisite to add the acetic acid in such proportion only as shall perfectly saturate the alkali holding the oxide of antimony in solution, for acetic acid appears to possess very little solvent power with respect to this oxide; for when muriate of antimony is dropped into strong acetic acid precipitation occurs exactly as it does in mere water.

The white residuum insoluble in muriatic acid was now washed; after being dried by the heat of a spirit-lamp, it weighed 28 grains = to 56 per cent. It was then mixed with carbonaceous matter, and heated slightly to redness; muriatic acid added to it readily gave a solution without the assistance of heat, which was decomposed by water. It is, therefore, evident, that the insoluble residuum was peroxide of antimony, which, by treatment with carbonaceous matter, was reduced to the state of protoxide, and rendered soluble in muriatic acid.

The phosphate of lime precipitated from the muriatic acid by the potash, washed and dried, weighed 21·1 grs. = 42·2 per cent.

From the experiments now detailed, it appears that James's powder is a mixture of

Peroxide of antimony.	56·0
Phosphate of lime.	42·2
Oxide of antimony, impurity, and loss. .	1·8
	100·0

Upon referring to Dr. Pearson's analysis, it will be observed, that when he heated 50 grains of James's powder with muriatic acid, only 14 grains remained undissolved by the acid, which is precisely half what resulted in my experiment. This difference is the more remarkable, because my statement of the composition of this powder agrees almost precisely with that given by Dr. Pearson, viz.

Oxide of antimony	57
Phosphate of lime	43
	100

The greater solubility of the oxide of antimony stated by Dr. Pearson, would seem to render it probable that the medicine in question was originally prepared in a different mode from that now adopted, and it is certainly possible that it may have formerly contained protoxide of antimony, of which it is now destitute. As now prepared, it differs from the pulvis antimonialis of

the Pharmacopœia in containing about one half more peroxide of antimony; but as it is questionable whether phosphate of lime is not as active as this peroxide, the difference of composition, though great in figures, can be but little in fact. When I allude to the composition of the pulvis antimonialis, it is of course to be understood that I speak of the result of my own analysis; but I shall presently adduce authority to show, that instead of being merely an inert preparation, it possesses the greater inconvenience of extreme uncertainty.

I cannot close this account of James's powder without alluding to some remarks which Dr. Paris has made in the last edition of his Pharmacologia (vol. ii. p. 357), upon my analysis of the pulvis antimonialis; and I hope the reader will excuse my quoting the observations at length.

"While correcting the present sheet for the press, a paper has appeared in the *Annals of Philosophy* for October, 1822, by Mr. Richard Phillips, of too important a character to be passed over without notice, as not only raising a question with respect to the chemical composition of this powder, but with regard to its medicinal efficacy.

"In consequence of the antimonial powder having proved inert in the hands of Dr. Elliotson, although exhibited to the amount of 100 grains for a dose, Mr. Phillips was induced to examine more particularly into the nature of the oxide which enters into its composition. 'After the well established fact,' says he, 'that *peroxide* of antimony is nearly or totally inert, it appears to me, that if proof could be obtained, that the oxide of antimony is in this state, the deficiency of power in the pulvis antimonialis would be accounted for.' He then proceeds to detail his experiments, from which he deduces the composition of this preparation to be as follows:

Peroxide of antimony	35
Phosphate of lime	65
	100

which exist together in a simple state of mixture. Until the subject be elucidated by further experiments, it will be difficult for the chemist to persuade the physician, that he can never have derived any benefit from the exhibition of antimonial powder."

For additional evidence as to the nature of this preparation, I beg to refer Dr. Paris to a statement respecting it, which has been made by Mr. Brande, and which, if I had remembered, would have saved me the trouble of an analysis. "In examining," says Mr. Brande, "the antimonial powder from various sources prepared according to the direction of the Pharmacopœia, I have found it of very variable composition: sometimes it contains peroxide of antimony only; sometimes there is a proportion of protoxide, and, in some few cases, the powder has con-

sisted chiefly of bone earth. These differences are referable to the mode of preparing it; but in almost every case, a very large proportion of the protoxide is lost during the process, and I have found it a matter of great difficulty so to conduct it as to obtain upon the large scale an uniform product. For medical use, I should consider emetic tartar as the only certain and necessary preparation of antimony."—(Manual, vol. ii. p. 180.)

It will be observed that without recollecting Mr. Brande's recommendation of emetic tartar to the exclusion of all other antimonials, I ventured to give similar advice; and I would conclude with remarking, that if it be possible to urge satisfactory reasons against a preparation, such reasons are contained in the passage I have quoted from Mr. Brande. It shows that pulvis antimonialis may consist almost entirely of phosphate of lime, or that it may be a mixture of phosphate of lime, and peroxide or protoxide of antimony; and it is a perfectly well-established fact, that fifty times as much peroxide of antimony may be given as of the protoxide; that the large dose is inert, the smaller one may be dangerous, and yet to this uncertainty is the physician exposed, although he may not be persuaded that "he can never have derived any benefit from the exhibition of antimonial powder."

Article V.

A List of the Plants found in the Neighbourhood of St. Petersburgh; taken from the Works of the Petropolitan Botanists. By Mr. J. B. Longmire.

Cl. I.—*Ordo Monogynia.*

Hippuris vulgaris.

Ordo Digynia.

Callitriche verna
——— æstivalis
——— autumnalis
——— intermedia.

Cl. II.—*Ordo Monogynia.*

Veronica officinalis
——— serpillifolia
——— verna
——— spuria
——— Beccabunga
——— scuttellata
——— Anagallis
——— Chamædrys
——— alpina
Pinguicula vulgaris
Lycopus europæus.

Ordo Digynia.

Anthoxanthum ordoratum.

Cl. 3. *Ordo Monogynia.*

Valeriana officinalis
Iris Pseud-acorus
Scirpus lacustrus
——— palustris
——— sylvestris
Nardus stricta
Eriophorum polystaochon
——— angustifolium
——— vaginatum
——— alpinum.

Ordo Digynia.

Phleum pratense
——— arenarium
Alopecurus pratensis
——— geniculatus
Aira cæspitosa
Agrostis rubra
——— Spica venti
——— capillaris
Aira alpina
Melica nutans
——— cærulea
——— palustris
——— nemoralis
——— maritima.

Ordo Digynia.

Poa annua
—— pratensis
—— trivialis
—— compressa
—— aquatica
Briza media
Dactylis glomerata
Cynosurus cristatus
Festuca duriuscula
——— fluitans
——— ovina
——— Secalinus
——— mollis
——— arvensis
——— squarrosus
——— sterilis
——— squarrosus var.
Arundo epigejos
——— stricta
——— Calamagrostis
Lolium temulentum
Avena sativa
Triticum repens
——— var. aristatum.

Ordo Trigynia.

Montia fontana
——— var. humilis.

CL. 4. *Ordo Monogynia.*

Scabiosa arvensis
——— succisa
Galium spurium
——— Aparine
——— palustre
——— Mollugo
——— boreale
Plantago major
——— lanceolata
——— media
Alchemilla vulgaris.

Ordo Digynia.

Cuscuta europæa.

Ordo Tetragynia.

Sagina erecta
Potamogeton gramineum
——— perfoliatum.

CL. 5. *Ordo Monogynia.*

Myosotis scorpioides
——— arvensis
——— var. præcox
Lythospermum arvense
Pulmonaria officinalis
Echium vulgare
Androsace septentrionalis
Convolvulus arvensis
Primula farinosa
——— veris
Menyanthes trifoliata
Lysimachia vulgaris
——— thyrsiflora
Hyoscamus niger
Polemonium cæruleum
Rhamnus Frangula
Campanula glomerata
——— rotundifolia
——— patula
——— persicifolia
——— Trachelium
——— rapunculoides
Solanum Dulcamara
——— nigrum
Lonicera Xylosteum
Ribes nigrum
—— Grossularia
—— alpinum
Viola palustris
—— ordorata

Viola officinarum
—— pallida
—— mirabilis
—— canina
—————— var. mont.
—— tricolor
—————— var. bicolor
—— arvensis
Impatiens Noli me tangere.

Ordo Digynia.

Chenopodium album
—————— glaucum
—————— rubrum
—————— hybridum
—————— urbicum
—————— viride
Ulmus campestris
Gentiana campestris
———— Centaureum
———— Amarella
———— Pneumonanthe
Conium maculatum
Heracleum Sphondylium
Selinum palustre
——— sylvestre
Cicuta virosa
Angelica sylvestris
Cherophyllum sylvestre
Æthusa cynapium
Carum carus
Pimpinella saxifraga
Aegopodium podagraria.

Ordo Trigynia.

Alsine media
Viburnum opulus.

Ordo Tetrandria.

Parnassia palustris
———— pentagynia
Linum catharticum.

Ordo Polygnia.

Myosurus minimus.

Cl. 6. *Ordo Monogynia.*

Juncus articulatus
——— bufoninus

Juncus bulbosus
——— conglomeratus
——— effusus
——— filiformis
——— pilosus
——— campestris.

Ordo Trigynia.

Rumex Acetosella
——— Acetosa
——— crispus
——— maritimus
——— acutus
Triglochin palustre.

Ordo Polygnia.

Alisma Plantago.

Cl. 7. *Ordo Monogynia.*

Trientalis europæa.

Cl. 8. *Ordo Monogynia.*

Epilobium montanum
———— var. rubens
———— palustre
———— angustifolium
Vaccinium Vitis idæa
———— Oxycoccos
———— uliginosum
———— Myrtillus
Erica vulgaris
———— var. alba.

Ordo Trigynia.

Polygonum viviparum
———— amphibium
———— Convolvulus
———— aviculare
———— persicaria
———— Hydropiper.

Ordo Tetragynia.

Adoxa Moschatellina
Paris quadrifolia
Elatine Hydropiper.

Cl. 10. *Ordo Monogynia.*

Monotropa Hypopithys
Arbutus Uva ursi

Ledum palustre
Andromeda polyfolia
———— caliculata
Pyrola secunda
——— rotundifolia
——— minor.

Ordo Digynia.

Chrysosplenium alternifolium
Gypsophylla muralis
Dianthus deltoides
Scleranthus annuus
Cucubalus Behen.

Ordo Trigynia.

Stellaria graminea
——— Dilleniana
——— crassifolia
——— nemorum
——— holostea
Arenaria peploides
——— rubra
——— trinervis.

Ordo Pentagynia.

Sedum acre
Oxalis Acetosella
Agrostema Gythago
Lychnis diurna
——— viscaria
——— vespertina
——— Flos Cuculi
Cerastium vulgatum
Spergula nodosa
——— arvensis.

Cl. 11. *Ordo Monogynia.*

Azarum evropæum
Lythrum Salicaria.

Ordo Digynia.

Agrimonia Eupatroria.

Ordo Trigynia.

Euphorbia escula
——— helioscopia.

Cl. 12. *Ordo Monogynia.*

Prunus Padus.

Ordo Pentagynia.

Spiræa Ulmaria.

Ordo Polygnia.

Rosa canina
Rubus Chamæmorus
——— arcticus
——— saxatilis
Fragaria vesca
Potentilla anserina
——— norvegica
——— argentea
——— opaca
Tormentilla erecta
Camarum palustre
Geum urbanum
——— rivale.

Cl. 13. *Ordo Monogynia.*

Actæa spicata
Chelidonium majus
Tilia europæa.

Ordo Trigynia.

Delphinium Consolida.

Ordo Polygnia.

Anemone nemorosa
——— ranunculoides
——— hepatica
Thalictrum aquilegifolium
——— flavum
——— angustifolium
Ranunculus reptans
——— Flammula
——— Ficaria
——— repens
——— sceleratus
——— auricomus
——— acris
——— polyanthemos
Caltha palustris
Trollius europæus.

Cl. 14. *Ordo Gymnospermia.*

Mentha arvensis
Nepeta cataria
Glechoma hederacea
Lamium perfoliatum

Lamium purpureum
——— album
Galeopsis Tetrahit
——— Ladanum
Clinopodium vulgare
Thymus Acinos
——— Serpyllum
Prunella vulgaris
Scutellaria galericulata
Origanum vulgare
Stachys sylvatica
——— (arvensis) palustris
Leonurus Cardiaca.

Ordo Angiospermia.

Euphrasia Odontides
——— officinalis
Rhinanthus Crista galli
Antirrhinum Linaria
Pedicularis palustris
Limosella aquatica
Melampyrum nemorosum
——— sylvaticum
Scrophularia nodosa
Linnæa borealis.

Cl. 15. *Ordo Siliculosa.*

Myagrum sativum
Bunias orientalis
Subularia aquatica
Draba muralis
——— verna
Thlaspi Bursa pastoris
——— arvense
Lepidium ruderale
Alyssum incanum.

Ordo Siliquosa.

Cardamine pratensis
——— amara
Sisymbrium Sophia
——— amphibium
Erysimum Barbarea
——— cheiranthoides
——— var. præcox
——— officinale
Arabis thaliana
Turritis glabra
Raphanus Raphanistrum
Brassica campestris.

Cl. 16. *Ordo Pentandria.*

Erodium cicutarium.

Ordo Decandria.

Geranium pratense.

Ordo Polyandria.

Malva rotundifolia.

Cl. 17. *Ordo Hexandria.*

Corydalis bulbosa
Fumaria officinalis.

Ordo Octandria.

Polygala amara
——— deflor.
——— vulgaris.

Ordo Decandria.

Orobus vernus
Lathyrus pratensis
——— palustris
——— sylvestris
Vicia sepium
——— Cracca
——— monantha
——— sativa
——— sylvatica
Ervum hirsutum
Trifolium rubens
——— repens
——— hybridum
——— arvense
——— spadiceum
——— agrarium
——— Melilotus albus.

Cl. 18. *Ordo Polyandria.*

Hypericum quadrangulare
——— perforatum.

Cl. 19. *Or. Polygamia Æqualis.*

Scorzonera humilis
Sonchus oleraceus
——— sibericus
Leontodon taraxicum
——— hispidum
Hieracium aurantiacum

Hieracium Auricula
——— dubium
———(Pilosella)umbelatum
——— pilosella
——— paludosum
——— cymosum
Crepis tectorum
Lapsana communis
Hypochæris rudicata
Serratula arvensis
Cnicus lanceolatus
Carlina vulgaris
Bidens tripartita
——— cernua.

Ordo Polygamia Superflua.

Artemisia vulgaris
——— campestris
Gnaphalium uliginosum
——— montanum
——— dioicum mas
——— fæmina
Tanacetum vulgare
Erigeron acre
Tussilago Farfara
Senecio vulgaris
——— paludosus
Solidago Virga-aurea
Inula dysenterica
ChrysanthemumLeucanthemum
Matricaria Chamomilla
Anthemis arvensis
——— tinctoria
Achillea Millefolium
——— Ptarmica.

Ordo Polygamia Frustranea.

Centaurea Jacea
——— Cyanea
——— phrygia
Filago arvensis.

Cl. 20. *Ordo Monandria.*

Orchis maculata
——— bifolia
———conopsea
Ophrys ovata
——— monorchys.

Ordo Monogynia.

Serapias rubra
Satyrium viride.

Cl. 21. *Ordo Diandria.*

Lemna polyrrhyza
——— minor
——— trisulca.

Ordo Triandria.

Carex filiformis
——— riparia
——— hirta
——— globularis
——— lævis
——— muricata
——— elongata
——— vulpina
——— arenaria
——— panicea
——— flava
——— limosa
——— cæspitosa.

Ordo Tetrandria.

Alnus incana
Urtica dioica
——— urens.

Ordo Polyandria.

Corylus Avellana
Betula alba.

Ordo Monadelphia.

Pinus sylvestris.

Cl. 22. *Ordo Diandria.*

Salix aurita
——— amygdalina
——— incubacea
——— purpurea
——— arenaria
——— pentandria
——— hermaphrodita
——— triandra
——— fusca
——— alba
Empetrum nigrum
Myrica Gale.

Ordo Octandria.

Humulus lupulus
Populus tremula.

Ordo Enneandria.

Hydrocharis Morsus ranæ
Mercurialis perennis.

Cl. 23. *Ordo Monoecia.*

Holcus odoratus
Atriplex hortensis.

Cl. 24. *Ordo Filicis.*

Equisetum arvense
——— palustre
——— limosum
——— hyemale
——— sylvaticum
Polypodium Phægopteris
——— Dryopteris
——— rigidum
——— Filix fæmina
Pteris aquilina
Osmunda Lunaria.

Ordo Musci.

Sphagnum palustre
——— var. rubens
Polytrichum juniperinum
——— commune
Mnium scoparium
——— hygrometricum
——— heteromallum
——— pellucidum
——— cuspidatum
Bryum viridulum
Hypnum serpens
——— filifolium
——— filicinum
——— dendroides
——— cupressi-forme
——— complanatum
——— proliferum
——— sericeum
——— repens
Orthotrichum commune.

Ordo Hepaticæ.

Marchantia polymorpha
Riccia crystallina
Conferva reticularis
——— rivularis
Jungermannia ciliata.

Ordo Lichenis.

Pulveraria chlorina
Lepraria incana
Opegrapha atra
——— obscura
Lichen islandicus
——— tenuissimus
——— physodes
——— parietinus
——— deformis
——— caninus
——— farinaceus
——— caperatus
——— hirtus
——— apthosus
——— atratus
——— cornutus
——— digitatus
——— paschalis
——— cocciferus
——— pyxidatus
——— rangiferinus
——— olivaceus.

Ordo Fungi.

Boletus versicolor
——— cinnamomæus
——— fumosus
——— perennis
Telephora hirsuta
——— terrestris
Merisma fœtidum
Clavaria abietina
Peziza abietina
Æcidium cornutum
——— Ranunculi
——— Ribesium
——— Convallariæ
Uredo Salicis
——— Alchemillæ
Xyloma salicinum
Sphæria argillacea
Tubercularia vulgaris
Rhizomorpha subcorticalis.

ARTICLE VI.

On the Existence of Chrome in the Ore of Platina.

(To the Editor of the *Annals of Philosophy.*)

SIR,

THE chemical history of platina must be considered to be as yet by no means complete. The last statement, with respect to the composition of its oxide, with which I am acquainted, is that of Mr. Cooper in No. 5, of the Royal Institution Journal, which differs widely from the statement of Berzelius. This has again been controverted, in a paper inserted in the *Annals* for November, 1821, the writer of which asserts, that the black powder, called oxide by Mr. Cooper, is in fact in the metallic state. This is a subject deserving of further investigation; * I am, therefore, desirous of learning (and, perhaps, some of your correspondents would inform me), whether the experiments of Berzelius are detailed in any English work? But the point to which I now wish to call your attention, is the existence of chrome in the ore of platina, a fact originally pointed out by Vauquelin, but upon which Tennant, by stating that he was unable to discover any, has thrown some doubt. An experiment which I have lately made completely verifies Vauquelin's statement, and at the same time seems to point out the reason why Tennant obtained no chrome, viz. that he operated only on the picked metallic grains of platina; while Vauquelin probably employed the crude ore, containing a quantity of black irony sand, in which the chrome is found.

In order to detect the presence of chrome, it is sufficient to separate the black sand by means of the magnet, and to expose it to a strong heat with carbonate of potash: chromate of potash is found in the crucible. To prove its nature, it was dissolved, neutralized, and tested with acetate of lead, when a yellow precipitate fell down: this precipitate being treated with muriatic acid, was resolved into a white salt and an orange liquid, which, after some boiling, turned green. Another portion of the precipitate, being properly fluxed, and exposed to a dull red heat, yielded the peculiar orange-enamel characteristic of chromate of lead: no doubt, therefore, could remain as to the nature of the substance. I am, Sir, your most obedient servant, C. C.

* I have repeated Mr. Cooper's process, which is to precipitate platina by nitrate of mercury, and to expel the calomel by heat; but as oxide of platina is decomposable at a heat below redness, I consider it impossible to stop the heat at the exact point, when all the mercurial salt is expelled, and all the oxygen of the platina retained, and, therefore, that the method can afford no satisfactory results.

Article VII.

Astronomical Observations, 1823.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Aug. 6.	Immersion of Jupiter's second satellite	15h 25′ 31″ Mean Time at Bushey. 15 26 52 Mean Time at Greenwich.

Article VIII.

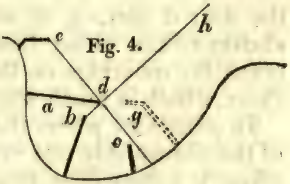
Essays on the Construction of Sea Harbours.
By Mr. J. B. Longmire.

(*Concluded from* p. 15.)

SIR, *July* 18, 1823.

With regard to the right direction of the piers, so as to throw the most of the surf to the shore, and as little as possible towards the mouth of a harbour; it is necessary first to ascertain the direction of all gales, that raise heavy surves at that place. Land gales at a harbour are harmless in this respect; and so in general are all gales making less angles with the main shore than 15°; so that all dangerous gales as to surf, lie in about 150° directly in front of the harbour. But some situations have not more than 130° of strong surf, and the most leeward of such surf, at the mouth of a harbour facing the calmest quarter, makes an angle of 45° with the main shore.

Fig. 4.



Certain considerations have almost universally prevailed in forming harbours. A sheltered situation has been selected, either in a creek, or near a part of the main shore that projects into the sea. The mouth faces that side which is the least disturbed by sea gales. The same plan, or at least the same principle, has been adopted as to the piers; if in a creek, the main pier, *a*, fig. 4, begins at the abrupt side, and extends either in a line parallel to the main shore, or inclines a little inwards. This pier has the best direction for quieting the interior water, and preventing the surf from disturbing the entrance, both being taken equally into consideration. But one pier alone placed in

any direction can neither sufficiently still the interior water, nor prevent the surf from accumulating at the entrance. For the completion of the former purpose, a secondary pier, *b*, is built, nearly at right angles to the main pier, extending to the shore from such a distance within the head of the main pier, and leaving an opening for the entrance of such a length, that the line *h a b* of the most leeward heavy surf, points to the outside of the secondary pier's head. These are the inclosing piers, and others for mooring vessels, &c. may be built in the basin thus rendered smooth.

To relieve the main pier from as much of the surf as possible, when the abrupt side extends further into the sea, a small covering pier, *e*, juts out from the extreme point of the land so far that a line, *e d*, drawn from its sea-end to the head of the main pier, makes an angle of 45° with the direction of the last pier. More than this, the main pier cannot be covered, without interfering too much with the lines of approach; but it keeps back the surves striking under angles of 15° to 45°, which, when strong, are the most dangerous surves.

Every surf acting against the secondary pier rebounds to the shore, so that a covering pier is not wanted to protect the entrance from its reverberated water. But heavy surves on a long range of lee-shore force a strong lateral agitation into the harbour. This can be considerably weakened by a very small pier, *c*, placed at such a distance from the secondary pier, that its sea-end shall not reach the line *e d*, when continued to the shore. This pier also lessens the quantity of surf in the space between it and the secondary pier, *b*, and so secures a safer retreat than the exposed shore, to vessels failing to enter the harbour. But a greater length of smooth shore is very desirable, as in particular gales vessels are sometimes driven past the pier, *c*. Hence also harbours gradually extended have obtained additional works, which make the inclosed lee-shore and entrance more or less similar to those represented by fig. 4, when the dotted pier, *g*, is added. In this state, vessels can take shelter within either head; but if driven too far in, they can reach the moorings on the innerside of the piers, or retire to the shore, which is much smoother than the exterior shore.

In situations where the main pier begins at the extreme point of the abrupt side of a creek, or extends further into the sea; or where it commences from a straight part of the main shore, stretches directly into the sea, and then turns to be parallel to the shore; a covering pier would be too expensive; and to prevent the reflected surf from accumulating too much at the sea end, the main pier, as in fig. 5, is built in parts not exceeding 100 yards each, having angles of 25° to 30°, with

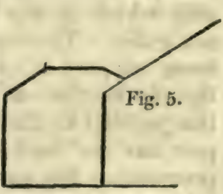


Fig. 5.

one another; and turning round bow-wise, sometimes with a head under a less angle. Much of the surf rebounding from one part, does not pass along the others; and what reaches the head is considerably less than would pass to it, if the pier was built in a straight line.

Fig. 6.

But one long main pier to form a harbour of the first kind, is not so complete as two main piers having an entrance between them, as in fig. 6. This certainly may be considered as two harbours facing each other, and rejecting the surf, but permitting it to pass into the space between them. It has a perfect entrance, as is shown in the first essay; but the other has not, having an open side to the lee-shore, and its entrance is less disturbed by the surf than the head of the long main pier.

Article IX.

Experiments for ascertaining the Velocity of Sound, at Madras in the East Indies. By John Goldingham, Esq. FRS.*

Between the years 1793 and 1796 a considerable number of observations were taken by myself, and under my superintendence, at the Observatory at Madras, with the view of ascertaining the velocity of sound. Not having the exact distances of the guns from the station when I returned to England, I wrote for further information upon the subject—which I had not obtained when I quitted Europe again. I therefore did not bring these experiments forward at the time; and having a more elevated station to observe from, by the erection of a new building, and the advantage of corroborating distances, by the trigonometrical survey carrying on under the superintendence of Col. Lambton, I entered upon the course of experiments about to be detailed. The former experiments (those of 1793 and 1796) were made with Arnold's chronometers, as were these now given. In examining works obtained from libraries here, since I closed these experiments, for information relative to the results of like experiments by other observers, I found a letter from Col. Beaufoy, in the *Annals of Philosophy*, addressed a few years ago to Dr. Thomson; and recommending to be done in England, what, in all the essential points, has been performed here, as will appear by the following extract:

"It has frequently excited my surprise, as well as regret, and in which I am no wise singular, that use has not been made of

* Abstracted from the Phil. Trans. for 1823, Part I.

the admirable Trigonometrical Survey, begun by the late Gen. Roy, and continued with so much ability and attention by Col. Mudge and Prof. Dalby, to make experiments on the velocity of sound; and however experiments of this kind may have been neglected, it is hoped that the present Master General of the Ordnance, a near relation of the late scientific Capt. Phipps (afterwards Lord Mulgrave) will, for the purpose of perfecting a branch of science, no less curious than useful, order a series of experiments of this nature to be undertaken, not only in the inland parts of the kingdom, but also on different parts of the Coast." He then mentions that the experiments should be made under different circumstances of the wind and weather, and at different times of the 24 hours, and proceeds to enumerate the stations where the experiments should be made. He recommends that pocket chronometers should be used, 'which, generally, making five beats in two seconds, the velocity of sound could be determined to the fraction of a second;' and concludes by saying, 'he has no doubt scientific foreigners would assist our countrymen in finding the time sound is travelling across that part of the Channel, where the shores are visible from each other."*

At Fort St. George (Madras) a morning and an evening gun are fired from the ramparts, as is customary in fortified places, the former at day light, and the latter at eight o'clock in the evening. At St. Thomas's Mount, the artillery cantonment, morning and evening guns are also fired, one at day light, and the other at sun set. The Madras Observatory, in latitude 13° 4′ 8″ north, is situated between these; the distance of it from the Fort, about half its distance from the Mount, the Fort being to the NE of the Observatory, and the Mount to the SW. In former years, as I have mentioned before, experiments were made by me for ascertaining the velocity of sound, but were not brought forward. And a new building,† elevated so as to give a commanding view of the country, particularly of the Mount‡ and Fort,§ having been erected, I commenced a new series with the morning and evening guns of both places. The experiments with the Mount gun, it will be seen, comprise an interval, which embraces all the varieties of the wind and weather during the revolution of the sun; the interval with the Fort gun is less, in consequence of the morning and evening guns having been fired from different parts of the ramparts, after the date at which the Fort experiments close. All the experiments were made with chronometers, which had 100 beats in 40 seconds, sometimes by three observers, myself and two of the Observatory Bramin assistants, but generally by two: the observers

* *Annals*, O. S. iv. 233.

† The station on this building is about 55 feet above the level of the sea, distant in a direct line 4500 yards.

‡ The Mount gun is about 120 feet above the level of the sea.

§ The Fort gun is about 30 feet above the level of the sea.

having repaired to the station at the top of the Observatory building, a little before the expected time, and each holding his chronometer so that he could distinctly hear the beats, began to count the instant he saw the flash, and continued counting until he heard the report; the number of beats between the flash and report was then immediately put down upon a slip of paper, by each observer, without communication with the others, and the papers delivered to me for their contents to be registered; the height of the thermometer, barometer, and hygrometer, with the direction of the wind and state of the weather, were also observed at the time, and registered; and in this manner the whole of the experiments were made. The situations of the guns with respect to the station from which the observations were taken, was very favourable, being in the direction, one of NE, and the other of the SW monsoons—with the southerly wind and sea breeze (both which prevail at certain seasons of the year), blowing between the two. The guns used were 24 pounders, charged with 8 lbs. of powder, and both pointed, not exactly towards the station, but in a direction not far from it.

The distances were ascertained with great care; first, by a survey made for the purpose, a base having been measured, and the angles taken with a grand circular instrument, similar to that used on the trigonometrical surveys.* Secondly, by using two or three of Col. Lambton's distances and bearings found by the trigonometrical survey.

The results were thus deduced, and verified in different ways; and I have reason to think that the distances of the guns from the Observatory station are very accurately given. The mean of twelve results made the distance of the Mount gun from the station 29547 feet; and the mean of six results gave the distance of the Fort gun from the station 13932·3 feet.

We see, as I before remarked, that the distance of one gun from the station is nearly double that of the other, and this will be found an advantage, in showing whether sound travels equally during its progress.

The experiments are given in eleven tables.†

Table I. Contains the experiments of each day with the Mount gun, together with the state of the atmosphere and the direction of the wind at the time of observation: the titles at the heads of the columns render a particular explanation unnecessary—the number of observers is stated in the third column, and the mean of their observations in the ninth.

Table II. Contains the mean of observations of each day, when the air was calm.

* I have not given the details of the survey, as that would swell the paper to an inconvenient size: the base, however, was measured with great care twice, and generally six observations were taken for finding each angle, each observation differing very little from the other.

† These Tables are necessarily omitted in this abstract.

Table III. The mean of observations of three days, when the wind was in the SE quarter.

Table IV. The mean of observations of three days, when the wind was in the NE quarter.

Table V. The mean of observations of three days, when the wind was SW by W, or NW.

Table VI. The experiments with the Fort gun, arranged as those in Table I., with the Mount gun.

Tables VII, VIII, IX, and X. The experiments with the Fort gun arranged according to the state of the wind, as in the former Tables of experiments with the Mount gun.

Table XI. Shows the mean motion of sound for each month at the Madras Observatory, as found by the experiments, at the mean height of the thermometer, barometer, and hygrometer, given in the table.

Upon a cursory inspection of Tables I. and IV. it will be seen that the motion of sound varies under different states of the atmosphere and weather: that, according to the first table, sound at one time has been as long as 27·6 seconds in travelling from the Mount to the Observatory station; and at another time only 24·8 seconds; the distance being 29547 feet. In the first case, therefore, the velocity of sound was only about 1078 feet in a second; while, in the other, its velocity was nearly 1191½ feet. The extremes in Table VI. show a still greater difference. This proves the necessity for making experiments during a long interval, in order to obtain an accurate general result.

In Tables II. and VII. we find, as the thermometer rose, the atmospherse at the same time decreasing in density and increasing in its elasticity, that the sound moved with greater rapidity.

That with the wind in the SE quarter the velocity was considerably increased, both from the Mount and Fort; more, however, in proportion, as might be expected, from the former than the latter.

That with the wind at NE. the sound from the Fort gun travelled with a greater, and from the Mount gun with a less velocity than when the wind was in any other direction; that wind being favourable for increasing the velocity from the Fort, and unfavourable from the Mount: the full effect of the wind, however, is not to be ascertained by this table alone, as the thermometer during the time the NE wind prevails is comparatively low, and the barometer high; both which, as will have been seen by inspection of the tables, occasion the sound to travel slower than ordinary.

The wind SW. W. and NW. the velocity from the Mount was accelerated, and that from the Fort retarded; but not in the degree that would have taken place had the thermometer, baro-

meter, and hygrometer, remained the same as in the NE monsoon; but having been different, the velocity was accelerated from both guns on this account, in like manner as it was retarded in the NE monsoon.

The following are the results deduced from the experiments in the different tables. I shall first give the general results from Table I. and VI.

TABLE I.

Mean height of Barometer.	Thermometer.	Hygrometer.	Seconds.	Distance.	Velocity in a Second.
Inches.				Feet.	Feet.
29·992	84·11°	19°	25·869	29547	1142·18

Or almost precisely the same as the velocity by the theory.

TABLE VI.

Barometer.	Thermometer.	Hygrometer.	Seconds.	Distance.	Velocity in a Second.
Inches.		Dry.		Feet.	Feet.
30·065	80·47	11·4	12·306	13932·3	1132·14

Here we find a difference from the former general result by the observations with the Mount gun; the reason of which appears to be, that I could not, as I have before stated, carry on the observations during at least a complete revolution of the changes in the atmosphere; and that this is the reason I shall now endeavour to show. The interval wanting is between the 28th of March and the 16th of July. Had this interval been wanting in the experiments with the Mount gun, there would have been a difference of 0·237 seconds in the mean result; for the mean of the experiments in this interval is 25·632″, and the mean of the whole 25·869″, making the difference just mentioned.

Now 25·869″ + 0·237″ = 26·106″, which would have been the mean number of seconds had the observations with the Mount gun been continued during the same interval only as the experiments with the Fort gun. Then 26·106″ : 0·237″ :: 12·306″ (the mean of the Fort observations) : 0·112″. Now 12·306″ − 0·112″ = 12·194″, which would have been the general mean of the experiments with the Fort gun, had the same been continued as long as the experiments with the Mount gun. Then the distance 13932·3 feet, divided by 12·194, will give 1142·5 for the motion of sound by the experiments with the Fort gun thus brought on; and this also agrees, within a fraction of a foot, with the velocity according to Sir Isaac Newton; and with the results by the two other celebrated philosophers before named (Halley and Flamsteed).

	Feet.	
We then have by the Mount gun	1142·18	for the velocity.
And by the Fort gun	1142·5.	

The mean is 1142·34, or very nearly the velocity above alluded to. Nothing could be more satisfactory than this general result;* and it may be presumed, that the other results in different states of the atmosphere are equally to be depended upon.

The velocity also by the Fort gun, which, it will be recollected, is little more than half the distance of the Mount gun from the station, shows that sound travels equally during its progress.

In the NE monsoon, the sound was very indistinct at times: this however does not appear to have sensibly affected its motion. The French academicians indeed proved, that this made no difference in the velocity.

I shall now proceed to the conclusions from the other Tables; and first, those of the experiments with the Mount gun.

Tables.	Barom.	Therm.	Hygrom.	Wind.	Seconds.	Dist.	Velocity in a sec.
	Inches.					Feet.	Feet.
II.	29·990	83·95°	20·31°	Calm	25·712″	29547	1149·2
III.	29·972	85·5	19·96	SE	25·754		1147·2
IV.	30·113	81·7	10·9	NE	26·812		1102·0
V.	29·934	85·1	26·0	SW. W. & NW.	25·374		1164·4
				Secondly, the experiments with the Fort gun.			
VII.	30·111	79·3	11·85	Calm	12·313	13932·3	1131·5
VIII.	30·023	82·3	14·6	SE	12·231		1139·1
IX.	30·131	78·6	7·33	NE	12·340		1129·0
X.	29·979	81·9	11·41	SW. W. & NW.	12·46		1118·1

The results in these Tables, like the separate observations, show the necessity of making a series of experiments long continued, in order to obtain the correct general rate at which sound travels; and this may afford a clue, as I observed in the first part of this paper, for discovering the cause of the differences in the results by the authorities there named: it is difficult, undoubtedly, to ascertain the distance of two stations, one far from the other, to the nearest foot; but errors of many feet in this respect, would make but a small difference in the velocity in a second found by experiment, when the gun and station were even at a moderate distance;† we must, therefore, be led to conclude, that these differences have chiefly arisen from the

* The results by the Mount gun may however be taken as the standard.

† For example, a difference of about 26 feet in the distance, between the Observatory station and the Mount gun, would make only about a foot difference in the velocity in a second.

experiments having been made during a limited period only, and at unfavourable times for obtaining a mean result, instead of the interval which appears by these experiments to be necessary.

A particular examination of the Tables and results will show the difficulty of ascertaining what proportion of the differences should be allowed to each of the instruments used for finding the state of atmosphere, exclusive of the effects of the wind.

During the calms, we might expect that the proportional parts to be allowed for the difference in the thermometer, barometer, and hygrometer, might be found with some degree of accuracy; the discrepancies, however, are very considerable. Comparing the results of Tables II. and VII. we find the barometer 0·121 lower, the thermometer 4·6° higher, and the air about $8\frac{1}{2}$ more dry by the former Table than by the latter, while the velocity in a second is only 17·7 feet greater by one Table than the other.

We give, however, in addition the following results taken from the Tables of calms, and arranged according to the different heights of the thermometer and barometer. These results may assist us in coming to some conclusion upon this part of the subject.

Experiments with the Mount gun.*

Barometer.	Thermometer.	Hygrometer	Seconds.	Distance.	Velocity in a second.
Inches.				Feet.	Feet.
30·109	88·13°	26·4°	25·97	29547	1137·7
29·889	88·0	28·4	25·45		1160·9
30·140	77·16	11·5	26·3		1123·4
30·089	81·3	11·3	26·40		1119·2
29·915	84·96	20·3	25·81		1144·7
29·93	82·12	16·0	25·91		1140·3
30·046	82·9	18·9	25·75		1146·5
		With the Fort gun.			
30·163	86·3	23·8	12·27	13932·3	1135·5
30·135	74·1	13·8	12·72		1095·3
30·063	80·76	8·8	12·11		1150·5
30·147	77·5	10·8	12·37		1126·3
29·943	82·25	15·0	12·15		1146·7
30·078	82·4	10·4	12·35		1128·1

Where the changes are so numerous and so frequent as in the atmosphere of the earth, we cannot expect that our imperfect instruments will be of a construction sufficiently delicate to show accurately every alteration that may affect the motion of the pulses of the air; but by various comparisons and combinations of the results, we may hope to arrive at general conclusions, somewhat approaching the truth.

* These are deduced from 100 observations.

Now, by numerous combinations of the observations just given, when the air was calm, we are led to conclude: first, that for each degree of the thermometer 1·2 feet may be allowed in the velocity of sound for a second; for each degree of the hygrometer 1·4; and for one tenth of an inch of the barometer* 9·2 feet. Then taking these numbers as the basis of the comparison, we find the mean difference of the velocity between a calm, and in a moderate breeze of wind, to be nearly 10 feet in a second. And by comparing other results together, a difference of about $21\frac{1}{4}$ feet in a second, or 1275 in a minute is found between, the wind being in the direction of the motion of sound, or opposed to it.

Before I conclude these introductory observations, and explanations of the experiments, it may be proper to refer more particularly to Table XI. containing the mean motion of sound for each month of the year, by the experiments with the Mount gun, according to the state of the atmosphere indicated by the different instruments; and to the prevailing monsoons, which may be considered to be the same, during the same months, every year; full information respecting which is given in the former Tables. On examining this Table, it is rather curious to observe how regularly the mean velocity proceeds to a maximum about the middle of the year, and afterwards retraces its steps; giving us a velocity in one case 1164 feet in a second, and in the other of only 1099 feet. This regularity would, no doubt, be still greater with the mean of the observations of several years.

Table XI.

Mean Motion of Sound for each Month, according to the Experiments with the Mount Gun.

Months.	Mean height of			Velocity in a second.
	Barometer.	Thermom.	Hygrom.	
	Inches.		Dry.	Feet.
January....	30·124	79·05°	6·2	1101
February ..	30·126	78·84	14·70	1117
March.....	30·072	82·30	15·22	1134
April ...	30·031	85·79	17·23	1145
May	29·892	88·11	19·92	1151
June......	29·907	87·10	24·77	1157
July.......	29·914	86·65	27·85	1164
August....	29·931	85·02	21·54	1163
September .	29·963	84·19	18·97	1152
October....	30·058	84·33	18·23	1128
November..	30·125	81·35	8·18	1101
December..	30·087	79·37	1·43	1099

* The rise and fall of the barometer is very limited in this country, as will be seen by an examination of the Tables. A sudden fall of 0·3 inch indicates a gale of wind.

Article X.

On newly discovered Animal Acids. By M. Chevreul.*

M. Chevreul has described five new animal acids, to which he has given the names of butiric acid, capric acid, caproic acid, hircic acid, and phocenic acid.

The *butiric acid* is the odorous principle to which soap, made with the butter of cows' milk, and the butter itself, more particularly owe their smell, but not entirely, for these bodies contain the capric and caproic acid, which also impart some odour to them. Butiric acid has, however, by much the strongest odour, resembling, when concentrated, the smell of strong butter and acetic acid; but when the acid is dilute, it smells like butter. The taste of this acid is at first hot, and afterwards sweetish, resembling that of nitric and muriatic ether. The butiric acid is colourless and fluid, and does not solidify at 15° of Fahr. and at 77° its specific gravity is 0·9675. In the state of hydrate it requires a higher temperature to boil it than water, and distils unchanged. It unites in all proportions with water, and when diluted with half its bulk of water, its specific gravity is greater than that of water. Alcohol combines with it in all proportions. When mixed with hogs'-lard, the butiric acid gives it the smell and taste of butter; the lard soon loses its smell by exposure to the air. In volume it is composed of

Oxygen	3
Carbon	8
Hydrogen	11

One hundred parts of this acid saturate 97·58 of barytes. An atom of hydrogen = 1, and of barytes 78; the weight of the atom of butiric acid must be nearly 80.

If the analysis had yielded 12 instead of 11 volumes of hydrogen, the composition of this acid would be 3 atoms of oxygen = 24, 8 of carbon = 48, and 6 of water = 6; the weight of its atom would consequently be 78, instead of nearly 80, as deduced from the composition of butirate of barytes.

Butirate of lime resembles its base in being more soluble in cold water than in hot; the butirate of barytes crystallizes in long prisms; 100 parts of water dissolve 36 of this salt.

Capric acid is obtained from the same sources as the butiric acid, and resembles it in being colourless, but it has a smell like that of a goat. In taste it is similar to that of the butiric acid. At 5° of Fahr. it exists in the form of small crystals; in the state of hydrate it requires a higher temperature to boil it than water does; it distils unaltered. The specific gravity of capric acid at 65° of Fahr. is 0·910; 100 parts of water dissolve only 0·12 of it, but with alcohol, it combines in all proportions.

* From the Annales de Chimie et de Physique, tom. xxiii. p. 16.

One hundred parts of it saturate 56·45 of barytes; the weight of its atom is, therefore, 138. The caprate of barytes forms small globular crystals; 100 parts of water dissolve 0·5 part of this salt.

Caproic acid is procured from the same substances that yield the butiric and capric acid. It is colourless, its smell is not so strong as that of the capric acid, but it is similar in taste; it remains liquid at 15° Fahr. and at 77°, its specific gravity is 0·923. One hundred parts of water dissolve 1·5 of it, and its hydrate distils unchanged at a higher temperature than water: with alcohol, it unites in all proportions. It is composed of

Oxygen	3 vols.
Carbon	12
Hydrogen	19

One hundred parts saturate 72·41 of barytes; its atom must, therefore, be represented by about 108.

Supposing the hydrogen to be 20 instead of 19 volumes, the composition of this acid would be 3 atoms of oxygen = 24, 12 atoms of carbon = 72, and 10 of hydrogen = 10, and the weight of its atom 106.

Caproate of barytes, when the solution evaporates spontaneously, crystallizes in needles, but if evaporated, it crystallizes at a lower temperature in hexagonal plates.

The *hircic acid* is the odorous principle of soap made of mutton suet; it exists in so very small a quantity, that fewer experiments have been made upon it than upon the preceding acids. It forms an hydrate, which is but little soluble in water, and does not solidify at 32° Fahr. Its smell resembles that of the goat. With barytes, it forms a salt of difficult solubility, while with potash it produces a deliquescent compound. It is this principle which gives mutton broth its peculiar odour.

Phocenic acid is the odorous principle of fish oil soap (*savon des huiles de dauphin*). It is colourless; remains fluid at 24° Fahr. It has a much stronger smell than either the capric or caproic acids. Its hydrate boils at a temperature above that of water, and distils unchanged. Its taste resembles that of those already described. At 77° its specific gravity is 0·932; 100 parts of water dissolve 5·5 of phocenic acid. It consists of

Oxygen	3 vols.
Carbon	10
Hydrogen	14

One hundred parts of this acid saturate 82·77 of barytes. Its atom must, therefore, weigh about 94. Phocenate of barytes is soluble in equal weight of water at 68° Fahr.: the crystals are large, and appear to be octahedrons.

From the analysis, this acid appears to be a compound of 3 atoms of oxygen = 24, 10 of carbon = 60, and 7 of hydrogen = 7, and the weight of its atom will consequently be 91.

Article XI.

On the Cause and the Effects of an Obstruction of the Blood in the Lungs. By David Williams, MD.

(To the Editor of the *Annals of Philosophy.*)

Liverpool, Aug. 5, 1823.

While investigating the effects of the pressure of the atmosphere upon the lungs, on its admission into the cavities of the chest, I remarked several appearances that militated against every hypothesis advanced, as to the cause of the unequal distribution of the blood after death. Reflecting on what I had witnessed, and thinking I had observed a phenomenon that had escaped the attention of all the physiologists whose writings I had perused, it encouraged me to a further inquiry. The result of my inquiry has been favourable, as it will, in my opinion, unveil the mystery that envelopes the cause of the comparative vacuity of the system circulating arterial blood *post mortem.* Before entering into the detail of my research, it will be better to premise the nature of the appearances alluded to. In one of my examinations, after the animal had been suffocated, by making a ligature on the trachea, during the acme of inspiration, previous to removing the sternum, I noticed after the action of the heart had ceased, that the blood still flowed into the right auricle and ventricle, and consequently into the pulmonary artery; and that the propelling agent was so powerful as to distend the right auricle and ventricle so forcibly after the pericardium was slit open, as to make it doubtful whether they would not burst, yet at the same time the pulmonary veins were comparatively empty. In this instance it was apparent, that the blood was obstructed in its course through the lungs, and that this obstruction was one of the principal causes of the vacuity of the circulating system of the arterial blood. From the distention of the cavities of the right side of the heart, and the gorged state of the cavæ, it was evident that no obstacle impeded the return of the blood through the capillaries, from the system at large. In a mechanical point of view, the blood ought to have met with equal impediment in passing through the capillaries, as in passing through the final terminations of the pulmonary artery into the pulmonary veins. Impressed with the comparative emptiness of the pulmonary veins, and as no visible subsidence of the lungs had taken place, I was at a loss how to assign a cause for the obstruction on a mechanical principle. It occurred to me that it was probable that the blood (from its vital principle being exhausted in its route through the system,

and from its supply from the thoracic duct being unassimilated), could not pass from the pulmonary artery into the pulmonary veins, without first being acted upon by pure atmospherical air. As such a cause seemed likely to offer a solution for every phenomenon connected with the subject, the idea was cherished, and for further satisfaction, the following investigations were instituted on the canine species. An animal was destroyed by securing the trachea at the acme of inspiration, afterwards the sternum and cartilaginous ends of the ribs were removed. The blood appeared florid in the pulmonary veins, and in the coronary arteries through the pericardium. When the contractions of the left ventricle began to flag, the pulmonary veins became less and less distended, the blood changing from the florid to a darker and darker colour as the currend diminished. At the last contraction the veins flattened, and the left ventricle felt contracted. At this instant, an irregular or fluttering contraction of the muscular fibres of the right ventricle commenced, and continued for a short time, excited seemingly by the stimulus of distention, from the accumulation of blood in its cavity. After the irregular muscular action had ceased, the right ventricle felt soft and distended, the left was still contracted, but not so rigid as immediately after the last systole. The pulmonary veins appeared empty; one of them was opened, when only a temporary oozing of blood followed. The pericardium was then slit open, and the right ventricle soon became enormously distended, yet no blood flowed out of the punctured vein. Another pulmonary vein was opened, followed by a similar oozing of blood. The pulmonary artery was now punctured, and instantaneously the blood gushed out, and deluged the shell of the chest. An animal was examined in the presence of Dr. Traill, after being destroyed in the same manner as the above, and the pulmonary veins were found in the same empty condition after the last systole.

From the investigation, the following corollaries are drawn:

1. That the blood is obstructed in its passage through the lungs, on suspension of respiration, while its circulation through the other parts of the body continues.

2. That the obstruction of the blood in the lungs, on suspension of respiration, is not the effect of a mechanical cause.

3. That the obstruction of the blood in the lungs, on suspension of respiration, arises from a deprivation of pure atmospherical air.

4. That the blood which is found *post mortem* in the left auricle and ventricle, is the remnant after the last systole, and the subsequent draining of the pulmonary veins.

5. That the obstruction of the blood in the lungs, on suspension of respiration, is one of the principal causes of the vacuity of the system circulating arterial blood *post mortem*.

6. That the immediate cause of the cessation of the action of

the heart, is a privation of its natural stimulus, arising from the obstruction of the blood in the lungs.

Among numerous phenomena observed in health and disease, which I conceive to arise from an obstruction of the blood in the lungs from a deficiency of pure atmospherical air, are the following. Hæmoptysis, in my opinion, is generally the effect of an accumulation of blood in the pulmonary artery, arising from a deficiency of pure atmospherical air in the lungs to decarbonate the blood, immediately on its being conveyed into that viscus. The deficiency may arise from an interruption of the action of the respiratory muscles, as from the immoderate use of the vocal organs, or from inspiring rarified and impure air, or from the over distension of the stomach, limiting the action of the diaphragm. Public speakers, singers, and performers on wind instruments, are well-known to be the frequent victims of hæmoptysis. The enthusiastic orator, stimulated by the interest of his subject, and proud of the approbation of his audience, endeavours, by every exertion, to make the greatest impression upon his hearers; by so doing he interrupts his respiration, and occasions a partial accumulation of blood in the pulmonary artery. If this interruption is often repeated, the minute branches of the pulmonary artery must become more and more dilated, as well as debilitated, and at last hæmoptysis will succeed; or, from habitual irritation, the foundation of a more insidious disease will be laid, I mean *tubercular consumption*. If the last conclusion be correct, we can account for the frequency of tubercular consumption in countries subject to sudden vicissitudes of the atmosphere. The consequence of sudden and frequent changes of temperature, must be sudden and frequent floods of blood, as it were, rushing into the lungs, especially into the lungs of those who have a delicate and a highly sensible constitution. The pulmonary arteries of open-chested persons easily accommodate those frequent torrents, as the blood from the capacity of their lungs is immediately exposed to the influence of the atmosphere, and undergoes the necessary change to admit it to proceed onwards without any delay. The pulmonary arteries of narrow-chested persons, on the contrary, soon feel the effects of a sudden increase in the circulating medium, for their lungs are unable to supply the increase of blood immediately with pure air, so as to enable it to proceed onwards without delay; therefore a temporary accumulation takes place in the pulmonary artery, which must irritate its extreme terminations.

Now I flatter myself, that the cause of the phenomenon that reserved the discovery of the circulation of the blood to modern times, and to the honour of our country, has been disclosed, and that no one for the future, however sceptical, will be able to

urge the vacuity of the arteries after death as an objection to the doctrine of our immortal Harvey.

How far temporary accumulations of blood in the pulmonary artery are a source of disease, I leave to the decision of time. Yet I must say, that Dr. Traill's coinciding with my views on the subject, has made me not a little sanguine, that my pathological speculations are founded upon a substantial basis ; and I cannot refrain acknowledging that I am gratefully sensible of my obligations to Dr. Traill, for his kindness during the above inquiry, as well as at all other times.

Article XII.

Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe. By the Rev. W. D. Conybeare, FRS. &c.

(*Continued from* vol. v. p. 359.)

Chalk Formation.

This formation appears to stretch through an area of great extent, occupying the interior of the grand European basin, reaching probably from the banks of the Thames to those of the Dniestr ; or if we attach credit to the observations of Dr. Clarke, even to those of the Don. It is not, however, to be understood, that its beds can be traced continuously throughout the borders of this area, so as to present an uninterrupted basset edge ; for this holds true of its western limits in England and France alone. In the central portions of Europe, it is greatly concealed partly by the overlying of the more recent tertiary deposits, and partly by the vast accumulations of diluvial debris, which veil from observation the native rocks throughout so large a portion of the north of Germany.

(A.) *Shores of the Baltic.*

The northern limit of this area may be traced in the line of the Baltic on the island of Rugen, where chalky cliffs present themselves on its northern coast, being found also on the neighbouring continent, in Pomerania and Mecklenburg. Hence the line appears to pass to the south of Sweden, where a small chalk tract occurs near Malmo, crossing to the opposite coast of Zealand, and including the small isle of Mona on the south. Some account of these localities may be found in De Luc's travels.

From Mona, the line of the chalk has not been traced: it probably traverses Holstein (where it is said to occur, probably

near the gypsum of Kiel) to the mouth of the Elbe, and thence crosses the German Ocean.

(B.) *England.*

This formation first exhibits those white cliffs which have been supposed to have bestowed on our island one of its ancient appellations at Flamborough Head, in Yorkshire; and thence stretching to the south-west, traverses England diagonally, till it reaches the British Channel, in Dorsetshire, being broken through, however, in its course by the æstuaries of the Humber and the Wash. The greatest breadth of this formation is in Wiltshire and Hampshire, where it expands into those vast plains which Pennant has appropriately termed the great central Patria of the English chalk. Hence it detaches two branches to the south-east, viz. the North Downs through Surrey and Kent, to the Dover and Folkestone cliffs, and the South Downs through Sussex, to those of Beachy Head. The interval between the North and South Downs is occupied by the formation of sand, &c. inferior to the chalk, constituting what has been called the denudation of the Weald, and extending into the Boulonais on the opposite side of the channel.

The areas lying between these branches and the main diagonal chain are occupied by basins of the more recent tertiary deposits, viz. the basin of London, between the main chain and the North Downs, and the basin of the Isle of Wight, between the main chain and the South Downs. The south side of this latter basin is skirted by a curvature of the main chain towards the east, deflecting it so as to cause it to run through the peninsula of Purbeck and the Isle of Wight. This deflected portion of the chain is remarkable from the circumstance, that its strata are throughout greatly elevated, and generally nearly vertical; while in other places the angle of the beds of this formation with the horizon rarely exceeds 2° or 3°.

The height of the chalky Downs in one instance (Inkpen, in Hampshire), exceeds 100 feet, and is often between 800 and 900 feet above the level of the sea.

(C.) *France, and the Netherlands.*

This formation occupies on the northern coasts of France, an extent exactly corresponding to its line on the southern coast of England. At the mouth of the Seine, its outer edge (which reposes on green sand, having oolite and lias in the neighbourhood) turns south, and so continues to Blois, where the formations above the chalk overlie and conceal its southern extremity: it reappears at Montargis, and turning again north (for the whole chalk district of France forms a sort of Cape protruding to the south of its general line), runs east of Troyes, Rheims, and Valenciennes, having the green sand, oolites, and lias, on its east, till it approaches the latter town, where most of these

formations are wanting (an instance of want of conformity in their direction), and the chalk, with a few beds of green sand, there called *Turtia*, rest horizontally on the truncated edges of the coal formation, which extends thence along the banks of the Meuse to Liege and Aix: the coal is here even worked beneath the chalk. North of Valenciennes, the edge of the chalk appears to trend to the east, but it is generally overlaid by the sandy superstrata through the Netherlands; it may, however, be seen on the south of Maestricht, and at Henri Chapelle near Aix.*

(D.) *Germany.*

As in the Netherlands we have traced the chalk skirting the north border of the coal fields which repose against the transition chains, so we find on crossing the Rhine the lower beds of the chalk formation (craie chloritée verdatre) similarly placed in the prolongation of these lines in Westphalia, i. e. to the north of the coal fields of the Rahn extending from Unna by Soist to Geseke and Lichtenau. Thence, after an interruption caused by the alluvia of the Lippe, it reappears near Domhagen and Paderborn, and forms at the foot of the muschelkalk a series of little escarpments, which extend by Schlangen, &c. beyond Hilter, in Osnaburg.†

To the north of the secondary hills of Westphalia, the whole district is well known to present the appearance of an uniform and vast sandy heath, covered with a deep accumulation of diluvial gravel, in the midst of which occur enormous rounded blocks of granite, for which a source cannot be found nearer than the opposite shores of the Baltic—thus exhibiting one of the most striking problems submitted to the investigation of geology. The great mass of this gravel, however, consists of chalk flints, well marked, and bearing traces of all the characteristic fossils: at least nine-tenths of the whole consist of these; a sign that the parent formation can be at no great distance. In such a tract, a rock *in situ* is like an oasis in the desert; at Luneberg, however, the fortifications are partly constructed on a rock of gypsum, and about a quarter of a mile hence, on the left of the road to Hamburg, the writer of this article was gratified by detecting a chalk-pit which had escaped the attention of former observers: it contains the usual alternation of flints, and affords good specimens of the inoceramus, echinites, and most of the characteristic fossils.

Dr. Bouè also notices a similar patch of chalk at Mount

* There are other chalky districts in the south-west of France connected with the basin of the Garonne, but these being apparently unconnected with the great chalky area, occupying the interior of the principal European basin, will be mentioned in the close of this article.

† I copy these localities from an excellent article of Boué's on the Geology of Germany, which appeared in the Journal de Physique for May, 1822. I have to regret that I was not earlier aware of the existence of this article.

Lindon, near Hanover, and several others between that town and Goslar, especially in the hills called Elbergebirge between Grasdorf and Unter Elbe. My friend, Prof. Buckland, informs me, that in this tract the chalk forms highly included ridges, like that called the hog's back, near Guildford. There are also several detached patches of the lower beds of this formation between Goslar, Halberstadt, and Enedlinburg (see Bouè). From their position these localities should seem to be occupied by outlying masses on the south of the general boundary of this formation; but Dr. Bouè mentions another point, Prenzlow on the Ucker See, in the north of Brandenburg, where it probably appears by denudation in the midst of the tertiary formations.

I do not here mention the chalk said to occur near Ratisbon which must be referred to a distinct basin, (that, namely, extending from the north foot of the Alps to the Bohemer Wald), nor, for similar reasons, the traces of this formation, which, according to Bouè, exist throughout the basin of Bohemia, and even in the valley of the Elbe, near Dresden, placing these as supplementary articles at the end of this sketch, of the course of the chalk through the principal European basin.

In pursuing then the southern boundary of this basin, it does not appear to have been noticed between those points north of the Hartz to which we have already traced it, and the district on the north of the Riesengebirge, where it reappears in Lusace and Silesia, e. g. on the west of Lawnberg and Lauben, &c.

(E.) *Poland.*

This formation here constitutes a line of hills running parallel to the Carpathians; it is finely exhibited at Cracow: it contains abundant flints, affords the usual organic remains, and rests on green sand: it was here examined by Prof. Buckland. Hence, passing by Lemberg, it appears to extend into Russia.

(F.) *Russia.*

The chalk is here exhibited according to the map of M. Beudant, in several detached points, on the north of the Dneistr to the north-east of Zaleszyky, between the 25th and 28th parallels of long. from London.

Hills of chalk were noticed by Dr. Clarke at Kasankaiya on the Don, and the town of Bielogorod, signifying the white city, is said to take its name from white hills of the same substance in its neighbourhood. Engelhardt observed chalk, containing its usual flints and fossils, even in the Crimea.

Mr. Strangways is, however, of opinion, from more recent examination, that the supposed chalk of the Crimea is really a tertiary formation, and that the localities on the Dniestr are the only ones which are well ascertained in Russia.

No particulars can be gathered of the eastern or north-eastern boundaries of this formation. We may conjecture, however,

that they pass by the Valday hills to the mouths of the Vistula; thence, the northern border must run eastward through the Baltic to the island of Rugen.

Chalk Deposits not immediately connected with the Great Central Basin of Europe.

(A.) *Ireland.*

In Ireland, a remarkable deposit of chalk forms the basis of the great basaltic area in the north-east angle of that island; it contains flints; the organic remains agree with those of England; the thickness of the whole deposit does not exceed between 200 and 300 feet; it rests on green sand.

(B.) *South-west of France.*

Chalk is said to occur on the borders of the tertiary basin of the Garonne, near Dex, on the south-west, and along its northern border.—(See the preceding article on green sand.)

(C.) *Spain.*

In Spain, chalk is said to occur near Cervera, on the road from Barcelona to Lerida; gypsum abounds in the same neighbourhood, and at Pleacente, two miles from Valencia, but the descriptions are too vague to be relied on; the gypsum mentioned seems to be rather that of the red sandstone, than of the formation above the chalk, and possibly a cretaceous marl may have been mistaken for the latter rock.

(D.) *Italy.*

In Italy, the *Scaglia,* which covers the extreme secondary chains of the Alps in the Veronese, may perhaps be a variety of chalk; it is described as a calcareous bed, containing nodules and beds of variously coloured flints, resting on the oolites and white limestones, and dipping under the tertiary hills (i. e. those consisting of the formations more recent than the chalk); it reappears against the volcanic group of the Euganean hills near the mouth of the Po, which appear to have forced it upwards.

(E.) *Basin of Bohemia, and the Valley of the Elbe.*

Dr. Bouè announces that the formation in this district, long known under the name of planer kalk, is really chalk. In the Valley of the Elbe, he has seen scattered patches of it in the bottom of a sinuosity in the granite near Mahles, on the east of Meissen; between Plauen and Strehlad, west of Dresden; at Colditz, and near Zchist, south of Pirna.

In Bohemia, between Toplitz and Bileu, and along the Laun to Lobositz and Grabern, sometimes supporting basaltic cones.

More to the south, this deposit appears to have formerly covered the coal and red sandstone formations over an area bounded by two lines, one passing from Hohenmouth to Prague,

Beraun, and Duckau; and the other from Eypel to Laun and Saatz, small patches being scattered over this district. On the confines of Bohemia and Moravia, especially between Hohenmouth and Tribau, it is still more abundant; it forms hills many hundred feet high on the north of Tribau.

It also occurs near Brisau and Lissitz.

(F.) *Basin of Swabia and Bavaria.*

This basin appears to exhibit cretaceous marls and chloritose chalk, like that of Bohemia on its southern border at the foot of the Alps, e. g. south of Munich, at Berg, and near Gastein.

(To be continued.)

Article XIII.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1823. Part I.

Upon perusing this part of the Philosophical Transactions, we find, with respect to several of the papers it contains, having already given such full reports of them as they were read before the Society, that we have little more to do in the present analysis than to refer the reader to those reports; correcting, however, as we proceed, a few slight inaccuracies in them, and supplying a few unavoidable omissions.

1. *The Croonian Lecture.—Microscopical Observations on the Suspension of the Muscular Motions of the Vibrio Tritici.* By Francis Bauer, Esq. FRS. FLS. and HS.—(See *Annals*, N. S. v. 66.)

"This minute animal, the vibrio tritici," Mr. Bauer informs us, "is the immediate cause of that destructive disease in wheat, known under the name of ear cockle, or purples, by farmers.

"On opening some of the diseased grains, I found their cavities filled with a mass of a white fibrous substance, apparently cemented together by a glutinous substance, and formed into balls, which could easily be extracted entire from the cavities of the grains, and which, when immersed in water, instantly dissolved, and displayed in the field of the microscope, hundreds of perfectly organized extremely minute worms, all which, in less than a quarter of an hour, were in lively motion."

In order to ascertain how these animals are propagated, and how they are introduced into the cavities of the young germens, the author "selected some sound grains of wheat, and placed some portions of the mass of worms in the grooves on the posterior sides of the grains, and planted them in the ground in the

month of October, 1807. Nearly all the seeds came soon up, and I took from time to time," he continues, "some of the young plants for examination, but could not perceive any effect of the inoculation, till the month of March, 1808, when, in carefully slitting open the short stalk of a young plant, I found three or four worms within it; they were in every respect the same, but they were now about two-thirds larger, as well in length as in diameter.

"On the 5th of June, I found, for the first time, some of the worms, of different sizes, within the cavities of the young germens; and having, in the beginning of March, found some of them in an enlarged state in the stalk, I concluded that some of the original worms, with which I had inoculated the grains of seed, had got, during the germination of the grains, into the stalk, where they became mature, and laid their numerous eggs, some of which must be carried by the circulating sap into the cavities of the then forming young germens, in which the young worms extricate themselves from these eggs; and finding their proper nourishment within the cavities of the germens, these young worms become of mature age, and lay their eggs within the cavities of these germens, which, at that period, nearly approach towards maturity; and these newly laid eggs, I consider to be the beginning of the third generation of the worms with which I had inoculated the grains planted in the ground in October, 1807.

"Towards the end of June, the germens assumed various distorted forms, and began to be filled with eggs. I extracted carefully the whole contents of one of the largest grains, and putting it into water in a watch-glass, I found, on examination under the microscope, seven large worms, all alive, bending and twisting in the water like so many small serpents."

The largest worms are more of a yellowish-white colour than the young ones, and are not so transparent; from the head, which is somewhat roundish, and furnished with a proboscis, as mentioned in our report of this lecture, they taper gradually off towards the tail, which is scarcely half the diameter of the middle of their body, and ends in an obtuse claw-like point.

"The movements of these large worms are very faint and slow; they are very seldom observed to unroll themselves entirely; they move their heads and tails faintly, but their proboscis they move constantly, extending and contracting it quickly; and when in the act of discharging their eggs, they bend the tail-piece upwards with a very quick jerk, at the passing of every egg; after having discharged all their eggs, the parent worms soon die, and in a few days they decay, and fall to pieces almost at every joint.

"The eggs come out from the orifice in strings of five or six, adhering to one another at their ends, which then appear truncated; but, in water, they soon separate, and assume an oval

form, which, in its middle, is slightly contracted. These eggs consist of an extremely thin and transparent membrane, through which the young worm can be distinctly seen; and, if attentively observed, it may be seen moving within this envelope."

The eggs, after the worms have quitted them, soon shrivel and decay, and it appears that they ultimately dissolve.

"The young worms are somewhat smaller and more transparent than those which are found in the more mature grains, but in a very short time after they have mixed with the others, they cannot be distinguished from them. Those which are found in the cavities of the mature grains, are nearly all of the same size; they are from $\frac{1}{33}$ to $\frac{1}{36}$ part of an inch in length, and $\frac{1}{1200}$ part of an inch in diameter. They are milk white, semi-transparent; and if viewed with the strongest magnifying power, appear annular, like the large worms, though no external indentations are observable; they appear like fine glass tubes filled with water, and containing many air bubbles in close succession, and of the same number as the rings or joints in the old worms. At both extremities (one of which is more sharply pointed than the other), there are no such divisions or joints perceptible. These extremities are each about one-eighth of the whole length of the worm; they are perfectly transparent, and appear like solid glass.

"The latter end of July, the diseased grains had almost all attained their full size, and assumed a brownish tint; and about the fifth of August they were all of a dark brown colour, variously distorted, and as hard as wood. The cavities of these grains were now completely filled with young worms, and these worms were, in every respect, the same as those with which I had inoculated my first seed grains; and those specimens were now more than twelve months old, and, consequently, the grains and the worms within them were completely dry; but after soaking them in water about an hour, the worms recovered their powers of moving, and were again as lively as those which were taken from the living plants.

"The large worms, after they become dry, die, and never revive; neither can the young worms within the eggs be revived, if the eggs have been but for a moment dry before the worms have extricated themselves." Mr. Bauer found that such worms as had been kept the shortest time in water, recovered their motions soonest; "so that those," he says, "which had been examined in the plain object-glass, where only a very small quantity of water can be applied, which very soon evaporates, almost every individual worm recovered in less than a quarter of an hour; and if the water is a second time suffered soon to evaporate, the experiment may be repeated many times successfully with the same worms; but after the second or third repetition, if there is a suspension of a week or ten days at each interval, several worms do not revive, and the number of these increases at every succeeding repetition. If this ex-

periment be not repeated too soon or too frequently, the worms retain their reviviscent quality much longer; the longest period of recovery, after a second suspension, I have hitherto ascertained, was eight months.

"If the worms are kept alive in water for a week or ten days, the experiment cannot be repeated so often, but the intervals of suspension may be prolonged considerably. I made the experiment very recently with grains which were three years and ten days old, and dry. After extracting the worms from the grains, I kept them in water 35 days, and after they had again been 15 days perfectly dry, I supplied them with water, and in less than twelve hours' soaking they were again, almost every individual, in as lively motion as if they had just been taken from fresh grains of the growing plant. I had the pleasure of showing these worms, in that state, to several Members of the Society, on the 29th of September last; after that day, I preserved the same specimens 18 days, perfectly dry; when, supplying them with water, I found, in less than three hours, at least one-third of them in lively motion; but the next morning, after they had just been 16 hours in water, they were all dead. If these worms are kept in a large glass, where the water cannot evaporate, they remain alive more than three months, but then they gradually die, and become as straight as needles." The glutinous substance in which the worms are preserved must be secreted by them, "since in grains in which the worms and the fungi or smutballs exist, that portion of the cellular tissue of the young germens, where a worm has formed its nest and laid its eggs, is entirely preserved; whilst in those portions of the grains which are immediately in contact with the fungi, the cellular tissue entirely disappears, and the fungi are only enveloped by the external tunic of the young germen."

This lecture is illustrated with two engravings from microscopical drawings by the author; one representing the diseased wheat, and the other the worms themselves.

II. *On Metallic Titanium.* By W. H. Wollaston, MD. VPRS.—(See *Annals*, v. 67.)

"My attention," Dr. Wollaston remarks, "has been directed, by various friends, especially by Professor Buckland, who gave me the subject of my experiments, to certain very small cubes, having the lustre of burnished copper, that occasionally are found in the slag of the great iron-works at Merthyr Tydvil, in Wales, which, from their hue, have, by some persons, been imagined to be pyritical. Their colour, however, is not truly that of any sulphuret of iron that I have seen; and though the form be cubic, it is not the striated cube of common iron-pyrites, which so often passes into the pentagonal dodecahedron, but similar to that of common salt; for any marks, that are to be discerned on their surfaces, appear as indented squares instead of striæ.

"Their hardness also is totally different from that of pyrites, and is such as, when combined with the preceding characters, marks a substance wholly unknown to mineralogists. By selecting a sharp angle of one of these cubes, I found that I could not only write upon the hardest steel, or upon crown glass, but could even visibly scratch a polished surface of agate on rock-crystal.

"Having broken out some of these crystals for experiment, I found them all apparently attracted by a magnet; but observing that they had still small portions of slag adherent to them, they were next digested in muriatic acid, which, by dissolving the iron from their surfaces, soon freed them from their deceptive appearance of magnetism.

"Before the blow-pipe they are utterly infusible. A continued heat oxidates them, and they become purple or red at the surface, according to the degree of oxidation, or depth to which it penetrates."

We must here add to Dr. Wollaston's statement respecting the purity of these cubes of titanium, as given in our report of this paper, that they contain no sulphur.

In considering the properties which evince that they are in a metallic state, Dr. W. observes, that when the action of nitre upon them is rapid, "heat is evidently generated, as by the combustion of other metals; but as I acted upon them in their solid state, and did not pulverise them, I did not witness what could properly be called detonation, as described by Lampadius."

To the several metals with which Dr. Wollaston was unsuccessful in his endeavours to unite one of these cubes, as already mentioned by us, we must now add lead. The following particulars form an appendix to this interesting paper.

"Since the date of this communication, the liberality of Mr. Anthony Hill, of Merthyr Tydvil, has supplied me with a larger quantity of the slag which formed the subject of my first experiments, and has enabled me to determine the specific gravity of metallic titanium to be 5·3. For this purpose, the vitreous part was fused with a mixture of borax and sub-carbonate of soda in about equal quantities, and was then dissolved in muriatic acid, which also removed a quantity of metallic iron, and left the titanium freed from extraneous matter. Though great part of what was thus obtained from the interior of the slag was in a pulverulent state, the quantity, which amounted to 32 grains, and displaced 6·04 of water, was sufficient to preclude any considerable error.

"I have moreover learned that metallic cubes, similar to those which I have above described and examined, were, more than 20 years since, observed in a slag at the Clyde Iron Works in Scotland; that a small quantity has also been met with at the Low Moor Iron Works, near Bradford, in Yorkshire; and at the Pidding Iron Works, near Alfreton, in Derbyshire; and that some good specimens have been obtained

from Ponty-pool, in Monmouthshire; but it does not appear that any one has ascertained, or even suspected, the real nature of this singular product."

III. *On the Difference of Structure between the Human Membrana Tympani and that of the Elephant.* By Sir Everard Home, Bart. VPRS.—(See *Annals*, v. 69.)

The full sound of the French horn, we find, produced the same effect upon the elephant at Exeter 'Change, with the low notes of the piano-forte, as described in our report of this communication.

We have already explained the difference of structure between the human membrana tympani and that of the elephant, as here described by Sir E. Home: his observations on that membrane in other quadrupeds are as follows; they are illustrated, together with the immediate subjects of the paper, by engravings from drawings by Mr. Clift.

"The nearest approach I have met with among quadrupeds to this peculiarity in the elephant, is in neat cattle: in them the membrane is more oval proportionably than in the elephant; it is $\frac{10}{20}$ of an inch long, $\frac{8}{20}$ broad. The handle of the malleus lies in the direction of the transverse diameter of the oval, and extends two-thirds of its length; it is not, however, situated in the middle line of the oval, but so much nearer to the anterior side, that the fibres on that side are two-thirds shorter than those on the opposite.

"In the deer, the membrane is of an oval form, whose transverse diameter is $\frac{7}{20}$ of an inch, the conjugate $\frac{5}{20}$: the malleus has its handle nearer the middle line than in neat cattle, the anterior fibres are $\frac{2}{20}$ of an inch, the posterior $\frac{3}{20}$ of an inch long.

"In the horse, and hare, the handle of the malleus lies in the middle line, so that the fibres on the two sides are equal. In the hare the handle is more curved.

"In the cat, the fibres are nearly the same as in the horse. I mention this circumstance, since it leads to the conclusion, that the whole of the feline kind have a similarly constructed organ.

"The effect of the high notes of the piano-forte upon the great lion in Exeter 'Change, only called his attention, which was very great. He remained silent and motionless; but no sooner were the flat notes sounded, than he sprung up, endeavoured to break loose, lashed his tail, and appeared to be enraged and furious, so much so as to alarm the female spectators. This was accompanied with the deepest yells, which ceased with the music."

IV. *Corrections applied to the Great Meridional Arc, extending from latitude* 8° 9′ 38″, 39, *to latitude* 18° 3′ 23″, 64, *to reduce it to the Parliamentary Standard.* By Lieut. Col. W. Lambton, FRS., &c.

This short paper, it is probable, forms the last communica-

tion respecting the measurement of an arc of the meridian in India, ever prepared for the scientific world by its lamented author; his decease took place, as we are informed by the Indian papers, on the 20th of January last, only eleven days after the present communication had been read before the Royal Society. We shall, on this account, be more particular in our analysis of these "Corrections," than, under circumstances of less interest, we probably should have been.

Col. Lambton first expresses his satisfaction at the results of Capt. Kater's experiments in examining and comparing the different standard scales; and his pleasure on finding "that the Commissioners for considering the subject of weights and measures have adopted Mr. Bird's scale of 1760, as by that means there is now a universal standard of comparison, which applies to the French metre, and to all the measures used on the Continent." "From Capt. Kater's results it appears," the Col. continues "that with respect to a measurement on the meridian, the degree depending on my brass scale must be multiplied by ,000018, and the product subtracted from the measure given by the scale, to reduce it to what it would have been, had it been measured by what is now the Parliamentary standard; and the degree depending on Ramsden's bar, by ,00007, and the product added to the measure given by the bar, to reduce it to the standard measure."

The arc which Col. Lambton measured, he next shows, depends on both these standards; and he then gives in succession its different sections, correcting them by the above factors as he proceeds. From these corrections we have the degrees as follows:

					Fathoms.	
"The degree	for latitude	9°	34′	44″ =	60477·09	Indian.
	for latitude	13	2	55 =	60490·31	Indian.
	for latitude	16	34	42 =	60511·65	Indian.
	for latitude	47	30	46 =	60779·00	French.
	for latitude	52	2	20 =	60824·26	English.
	for latitude	66	20	12 =	60955·00	Swedish.

"Then computing from Eq. 3, page 498, in the Phil. Trans. for 1818, Part II., we shall have the ellipticity of the earth as follows: by the

Indian and French, $\frac{1}{310,07}$; $\frac{1}{309,64}$; $\frac{1}{313,73}$; Mean $\frac{1}{311,15}$;
Indian and English, $\frac{1}{310,3}$; $\frac{1}{309,94}$; $\frac{1}{313,72}$; $\frac{1}{311,32}$;
Indian and Swedish, $\frac{1}{307,88}$; $\frac{1}{307,55}$; $\frac{1}{309,92}$; $\frac{1}{308,45}$;
General Mean $\frac{1}{310,31}$;

It is next shown, by means of a table computed from certain data given in the paper, "that the first degree in latitude 9° 34′ 44″ by the measurement is 0·67 fathoms in defect; and that the degree in latitude 16° 34′ 42″ (which may be taken for 16° 34′ 44″) by the measurement is 3·21 fathoms in excess."

With respect to the dimensions of the earth, and the length of the quadrantal arc of the elliptic meridian, the author then deduces, that 60850·17 fathoms is the measure of the degree on the equatorial circle; and that "5467756 fathoms is the length of the quadrantal arc, which, reduced to inches, and multiplied [divided] by 10,000000, we get 39·3677 inches for the metre at the temperature of 62°, which falls short of the French metre by ·0032 inches, when reduced to the same temperature."

"This conclusion is very satisfactory, and I hope that equal success will attend my operations to the northward. I have already measured another section, which extends to latitude 21° 6′, having just returned from finishing it; and when all the necessary calculations and corrections are made, I shall draw out an account of the whole, and forward it to the Royal Society at a future period. The celestial arc has been determined by seven stars, but there are many now out of my reach which I observed in the beginning.

"It may be satisfactory to the mathematicians in Europe to know, that I am now advancing through Hindoostan; and from what I can learn from the different public authorities, I do not apprehend any difficulty. They are all inviting in their letters, and all seem desirous that I should go through their respective districts. If my present arc be continued direct, it will pass through Bopaul, and near Seronje, where I shall have again to observe the stars, and measure a base; and if Scindiah's country be in a quiet state, my meridian will pass through Gualior, his capital; and my sixth section will terminate near Agra, on the Jumna. I have made up my mind to execute all this if I live, and continue to have that flow of health and spirits which have hitherto attended me. The result of such an extensive measurement must be interesting to scientific men; and I shall exert my endeavours in doing justice to the work, and in giving a faithful account of the operations."

In concluding our notice of this paper, we cannot but express onr earnest hope that some fully qualified person may speedily be appointed to continue Col. Lambton's operations, as well in the measurement of the arc, as in the extension from it of a general survey of the country; the latter undertaking we believe, had already been commenced by Col. Lambton, and the present state of our knowledge respecting the geography of Hindostan imperiously requires its prosecution.

V. *On the Changes which have taken Place in the Declination of some of the principal fixed Stars;*

VI. *Appendix to the preceding Paper on the Changes which appear to have taken place in the Declination of some of the fixed Stars;* and

VII. *On the Parallax of α Lyræ.* By John Pond, Esq. Astronomer Royal, FRS.

To give a satisfactory account of these important papers

would occupy a far greater space than we could at present devote to the subject: we must, therefore, confine our extracts, in this place, to the conclusion of the paper on the parallax of α Lyræ; the difference between which and that of γ Draconis, Mr. Pond finds, is absolutely a quantity too small to be measured, or it is zero; and his observations indicate, in the most decided manner, that the actual parallax of the former star cannot exceed a very small fraction of a second.

"Notwithstanding the importance of these investigations to the history of astronomy, and to our forming a correct notion of the system of the universe, yet our decision ultimately turns upon so very small a quantity, that our having reduced the inquiry to these narrow limits, rather tends to show the perfection of each instrument [of Greenwich, and of Dublin], than the defect of either."

"On former occasions, I considered the question of parallax in the particular case of α Lyræ as undecided, and as perfectly open to future investigation; but the observations of the present year have produced, on my mind, a conviction approaching to moral certainty. The history of annual parallax appears to me to be this: in proportion as instruments have been imperfect in their construction, they have misled observers into the belief of the existence of sensible parallax. This has happened in Italy to astronomers of the very first reputation. The Dublin instrument is superior to any of a similar construction on the Continent; and, accordingly, it shows a much less parallax than the Italian astronomers imagined they had detected. Conceiving that I have established, beyond a doubt, that the Greenwich instrument approaches still nearer to perfection, I can come to no other conclusion than that this is the reason why it discovers no parallax at all."

VIII. *Observations on the Heights of Places in the Trigonometrical Survey of Great Britain, and upon the Latitude of Arbury Hill.* By B. Bevan, Esq.: Communicated by Sir H. Davy, Bart. PRS.

By means of levelling to the canals, &c. Mr. Bevan found the country to the north of Arbury Hill suddenly to fall about 400 feet, and continue at this depressed state for nine or ten miles. This "defect of matter," he observes, "was a strong ground for supposing a deflection of the plumb-line to the southward;" and by calculating the latitude of Arbury station, from the latitude of Blenheim, as determined by previous observation, independent of any astronomical observation made at Arbury, he found it to be 52° 13′ 23″, or five seconds *less* than was shown by the zenith sector. For the calculation by which this discrepancy was deduced, and for the other subjects of the paper, we must refer the reader to the original. B.

(*To be continued.*)

Article XIV.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

June 20.—A paper was read, containing a Description of a Section of the Crag Strata at Bramerton, near Norwich. By Richard Taylor, Esq.: communicated by John Taylor, Esq. Treas. GS.

This paper was accompanied by a sketch of the crag beds at Bramerton, resting upon the upper chalk, and a table was subjoined containing the respective thicknesses of the series of beds, with a list of such organic substances as belong to each.

A paper was also read, on the Geology of Rio de Janeiro. By Alexander Caldcleugh, Esq. MGS.

The mountains in the neighbourhood of Rio de Janeiro are for the most part composed of gneiss intersected by granite veins.

A siliceous stalactite was observed by the author to form in this district from the overhanging masses of gneiss, specimens of which were presented to the Society.

As the absence of hot springs makes the occurrence of these stalactites of very considerable interest, Mr. Caldcleugh offers the following hypothesis to explain their formation; the water which in Brazil constantly trickles down the bare sides of the hills, often reaches a temperature as high as 140° or 150° of Fahr. This warm water descending on decomposing strata of gneiss, such as is the case with that from which these specimens are taken, seizes the potash of the felspar, and then acts upon the quartz, and forms a siliceous stalactite. Some of the hot springs or geysers of Iceland do not reach the boiling point, and perhaps the quantity of silex dissolved, the inverse of what is shown to be the case with carbonate of lime, may, in a great measure, depend on the temperature of the alkaline solvent.

June 27.—A paper was read, entitled, "Observations on the Quartz Rock Mountains of the West of Scotland and North of Ireland, more particularly those of Jura, with an Account of the ancient Beaches and Trap Dykes of that Island, accompanied by a Plan and Sections."

The quartz rock is traced in a succession of districts from Lerwick, in Shetland, to the county of Donegal, in Ireland; and in Jura the thickness of the mass is estimated at 10,260 feet. The similarity and singularity of form assumed by quartz rock mountains in districts remote from each other is deduced from the peculiar construction and material of the mountain mass acted upon by powerful aqueous currents. Quartz rock is of great extent in the county of Donegal, where, in one instance,

it rests immediately on granite, and at the Muckish mountain contains a bed of pure siliceous sand of considerable thickness.

The author proceeds to notice the ancient beaches of Jura which appear hitherto to have escaped observation: these occur on both shores of Loch Tarbert, and are disposed in six or seven terraces rising regularly from the present shore, above which the highest is elevated about 40 feet; the breadth occupied by these beaches, in some instances, amounts to three-fourths of a mile, and their line or extent has been traced eight or ten miles.

The author concludes with a description of, and remarks on, the trap dykes of Jura; these are extremely numerous, and remarkable for preserving courses nearly parallel to each other, and nearly in the line of dip of the quartz rock which they traverse, which gives occasion for offering some reasons to account for that particular disposition.

Article XV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Composition of Morphia.*

From the experiments of M. Bussy, it appears that morphia is composed of

Carbon	69·0
Azote	4·5
Hydrogen	6·5
Oxygen	20·0
	100·0

(Journal de Pharmacie, viii. 590.)

II. *Corrections for Moisture in Gases.*

The following observations and formulæ are taken from the last edition of Dr. Henry's Elements of Chemistry, vol. i. p. 25:—

Another correction, which it is often necessary to make in taking the weight of gases, is for the quantity of aqueous vapour diffused through them. It is obvious that all gases, which are specifically heavier than aqueous vapour, must have their specific gravity diminished by admixture with steam; and, on the contrary, all gases that are specifically lighter than steam must have their specific gravity increased by that admixture. For the following formulæ, I am indebted to Mr. Dalton, who has obligingly stated them at my request.

"At ordinary temperatures, the tension or elasticity of aqueous vapour varies from 1-100th to 1-50th of the whole atmospheric pressure; in the present case it is supposed to be a given quantity. The specific gravity of pure steam compared with that of common air, under like circumstances of temperature and pressure, is, according to Gay-Lussac, as 0·620 to 1.

"Let a = weight of 100 cubic inches of dry common air, at the pressure 30 inches and temperature 60° Fahr.; p = any variable pres-

sure of atmospheric air; and f = pressure or tension of vapour in any moist gas. Then the following formulæ will be found useful in calculating the volumes, weights, and specific gravities, of dry and moist gases; putting M for the volume of moist gas; D for that of dry gas; and V for that of vapour, all of the same pressure and temperature.

1. $M = D + V$.

2. $\frac{p-f}{p} M = D$.

3. $\frac{f}{p} M = V$.

4. $M = \frac{pD}{p-f} = \frac{pV}{f}$.

"If we wish to infer the specific gravity of any dry gas from the observed specific gravity or weight of the same mixed with vapour, it will be convenient to expound p by that particular value which corresponds with a, namely 30 inches of mercury; and let s = the specific gravity of the dry gas, and w = the observed weight of 100 cubic inches of the moist gas.

Then we shall have the following, viz.

5. $\frac{30-f}{30} . s\, a + \frac{f}{p} \times \cdot 620\, a = w$.

6. $s = \frac{30}{30 - f\, a} \left(w - \frac{f}{p} \times \cdot 620\, a.\right)$

Exemplifications.

1. 98 vol. dry air + 2 vol. vapour = 100 vol. of moist air.
2. Given $p = 30, f = \cdot 5$, and $M = 100$.

Then $\frac{p-f}{p} . M = D$, the dry air, $= 98\frac{1}{3}$.

3. And $\frac{f}{p} M = V$, the vapour, $= 1\frac{2}{3}$.
4. Given $D = 100$, $p = 30, f = \cdot 4$.

Then $\frac{30 \times 100}{29\cdot 6} = 101\cdot 35$, the moist air.

Given V* $= 2, p = 30, f = \cdot 3$.

Then $\frac{30 \times 2}{\cdot 3} = 200$, the moist air.

5. Let $f = \cdot 5$, $s = 1\cdot 111$, $a = 30\cdot 5$, $p = 29\cdot 5$,

Then $\frac{30 - \cdot 5}{30}\ 1\cdot 111 \times 30\cdot 5 + \frac{\cdot 5}{29\cdot 5} \times \cdot 62 \times 30\cdot 5 = 33\cdot 64 = w$, which gives the specific gravity 1·103.

6. Let f, a, and p as above, and $w = 2\cdot 5$, corresponding to specific gravity 0·8197.

Then $s = \frac{30}{29\cdot 5 \times 30\cdot 5} \left(2\cdot 5 - \frac{\cdot 5}{29\cdot 5} \times \cdot 62 \times 30\cdot 5\right) = \cdot 07266$.

"The above formulæ apply equally well if V be a permanent gas, or any other vapour beside that of water, the specific gravity of the gas or vapour being substituted instead of ·620 that of steam."

* It is easy to see that V, in this and the other cases, mostly will denote a virtual volume only; or such as would result, if the vapour were condensible like a gas, without being convertible into a liquid.

III. *Crystallized Steatite.*

According to the analysis of Prof. Dewey, this mineral contains

Silica	50·60
Magnesia	28·83
Oxide of iron	2·59
Oxide of manganese	1·10
Alumine	0·15
Water	15·00
Loss	1·73
	100·00

"In heating the mineral, there was sometimes more and sometimes less than 15 per cent. of water liberated; but the water is taken at 15 per cent." The above proportions, it appears, are between those obtained by Klaproth in his analyses of steatite from two localities; "there can be no doubt, therefore," Prof. Dewey remarks, "that these crystals are *real steatite.*"

"The *form* of some of these crystals, is that of a six-sided prism terminated by six-sided pyramids, often variously truncated. Some of them appear to be four-sided prisms terminated by a four-sided pyramid. They are unquestionably the crystals intended by Jameson, as they are found in a similar situation to those mentioned by him, though they seem not to be *pseudomorphous.* The locality is described, vol. v. p. 249, of this Journal. They are sometimes covered with a very fine grained and close brownish steatite, in which, as in the asbestus, the crystals leave their form. The specific gravity of the crystals is less than that given to steatite. In the various specimens I have tried, it has been found very nearly 2, sometimes a little more or a little less. Their specific gravity may be taken at 2, water being unity."—(Silliman's Journal, vi. 334.)

IV. *Earthquake and Volcanic Eruption in Java.*

On the 27th of December, a shock of an earthquake was felt at Java, and it was repeated 18 times in 30 hours. At the same time, a subterranean noise was heard in the mountain of Merapic, which began to eject stones. On the 29th, at one o'clock in the morning, an eruption took place, during which half of the mountain was surrounded with torrents of lava and columns of fire, while a heavy shower of sand and small stones covered the environs. The village was destroyed, and 15 persons perished. At the mountain of Bruno, a very strong subterranean noise was heard, and it began to eject small black ashes which were perceptible at a considerable distance.—(Journal de Physique, tom. 96, p. 80.)

V. *Glassy Actynolite.*

The characters and constituents of glassy actynolite from Concord Township, Delaware County, Pensylvania, as determined by Mr. H. Seybert, are as follows:

Colour, in the mass, emerald-green; powder greenish-white. Lustre vitreous. Translucent. Fracture in one direction fibrous; in the opposite irregular. Very frangible. Scratches glass. Structure

fibrous and fasciculated. Specific gravity 2·987. Fusible before the blowpipe into an opaque greenish enamel. It contains

Silica	56·333
Magnesia	24·000
Lime	10·666
Protoxide of iron	4·300
Alumina	1·666
Water	1·033
Protoxide of chrome	A trace
	97·998
Loss	2·002
	100·000

The loss of weight by ignition is estimated as water in this statement. —(Silliman's Journal, vol. vi. p. 331.)

VI. *Discovery of Mineral Caoutchouc in New England, United States.*

The following is Prof. Silliman's account of this discovery, as given in his journal, vi. 370:—

"This remarkable mineral, hitherto nearly or quite confined to the Odin mine at Castleton, in Derbyshire, has been recently found at Southbury, 20 miles north-west of New-Haven. This region is a secondary trap basin, and although only six or eight miles in diameter, it presents all the characteristics of the great trap region of Connecticut and Massachusetts described by Mr. Hitchcock. Among other things, it contains slaty rocks with bituminous minerals; these have induced a search for coal which is now going on. We understand that they find bituminous slate or shale with small veins of coal. Specimens confirming this statement are now on the table, and they exhibit fibrous limestone, forming very distinct veins, or rather layers, running parallel with, and lying between, those of the slate. The fibres of the satin spar or fibrous limestone are one inch or more in length; they are often cracked in the direction of the fibres, and between them there are veins occupied by the mineral caoutchouc. It has but little elasticity, it is soft, easily impressible by the nail, and compressible between the fingers like potassium, and can be formed into a perfect ball; its colour is jet black; some varieties of it are a little harder, and have a resinous and splendent lustre, and a flat conchoidal fracture; it burns with extreme brilliancy, with much black smoke, and an odour between that of a bitumen and that of an aromatic; during the combustion, drops of liquid fire fall in a stream, or in quick succession, and with a whizzing noise, exactly like the vegetable caoutchouc, and it melts precisely as that substance does. Rubbed on paper, it leaves a black streak, and acquires a high polish; it does not remove pencil marks from paper. The veins containing this mineral are about one-quarter of an inch wide, and several inches long."

VII. *On an Improvement in the Apparatus for procuring Potassium.* By W. Mandell, BD. Fellow of Queen's College, Cambridge.

"On repeating the late Prof. Tennant's experiment for procuring potassium (which differs from the similar one first made by the French chemists, Gay-Lussac, and Thenard, principally in being more simple

and commodious for practice), it occurred to me, that one part of the apparatus made use of, might, with advantage, be still further simplified; and as every circumstance, however apparently obvious, or trivial in itself, which, in any degree, tends to facilitate the production, in greater quantity, of so powerful a chemical agent as potassium, is of importance, I have thought that the mode of operating which I pursued might not be wholly unworthy the notice of this Society."

"It is well known that the grand difficulty in successfully performing the experiment in question, consists in protecting the gun-barrel from the effects of that extreme and long-continued heat, which is necessary to decompose the alkali, and to volatilize its base. The usual practice hitherto has been to surround with a *lute* that portion of the gun-barrel which is introduced into the fire. This operation, however, is always tedious; and although it be conducted even with the greatest care, it is found extremely difficult to prevent fissures in the coating, particularly when the heat is much increased in the course of the experiment. Hence, if eventually the fire have direct access to the barrel, through any crevice which may be formed, the fusion of the denuded part is generally the consequence, and the whole labour of the experiment is lost."

"This then being the common cause of failure, it occurred to me that, if there were substituted for the luting, a thin but sound and well-burnt *tube* or hollow cylinder of Stourbridge clay, of such dimensions as just to cover that portion of the barrel which is subjected to the fire, the unfortunate result, which I have alluded to, might possibly be avoided."

"A tube of this description was accordingly procured; and in order to guard against the hazard of its cracking, by reason of exposure to a sudden increase of temperature, it was, in the first place, gradually and with caution, heated to redness."

"The remaining part of the experiment was then performed with entire success; and a very considerable quantity of potassium obtained."

"It may be proper to remark that the hollow cylinder, and that portion of the gun-barrel which it incloses, should be of such relative diameters that, when cool, their corresponding surfaces are not quite in close contact; otherwise the cylinder will be in danger of bursting, not only on account of its own contraction, but also on account of the simultaneous expansion of the gun-barrel, from the effects of that very high temperature, to which, in this state of combination, they are submitted."

"Moreover, the whole apparatus should be supported accurately in the same position throughout the experiment (by means of rests made of Stourbridge clay), and should be so situate in the fire, that the materials operated upon may, during the whole process, be submitted to its greatest intensity."

"With due attention to these precautions, and to some minor circumstances in the manipulation of the experiment, which I shall not take up the Society's time in detailing, it is believed that the decomposition of potash, by means of iron, might, in every instance, be effected with almost entire certainty, and potassium be obtained in great abundance."—(Cambridge Phil. Trans. 1822. Part II.)

VIII. *Dr. Boué on the Newer Deposits of the Alps.*

In the first volume of the *Annals*, N. S. we published Prof. Buckland's "Notice on the Structure of the Alps, &c.;" and we now insert some descriptive remarks on a part of the same subject, by another eminent geologist, Dr. Boué, whose opinion respecting it has already been adverted to by the Rev. W. D. Conybeare, in his "Memoir on the Mountain Chains of Europe," *Annals*, v. 282, N. S. They are derived from Dr. Boué's "Outlines of a Geological Comparative View of the South-west and North of France, and the South of Germany;" read before the Wernerian Society on the 15th of April last, and published in the Edinburgh Philosophical Journal for July, p. 128.

"We shall now trace the shell limestone, and show, that by some observers it has been confounded with the zechstein. It may afford matter of surprise that I should contradict the opinion of so many celebrated men, but the fact is clear, and the confusion has arisen merely from mistake regarding the geognostical position of the Jura limestone. In Swabia, geologists not finding the zechstein, and yet being anxious to recognize a deposit so well known in the north, had naturally, from their not being acquainted with the shell limestone (muschelkalk), taken this deposit for the zechstein, because it lies above what they rightly consider as the todliegende. This base admitted, they naturally believed that the salt deposit was placed between their zechstein and todliegende, and this salt they rendered subordinate to the zechstein or alpine limestone of Friesleben. Further, they then naturally called the Jura limestone the shell limestone (muschelkalk), and the quadersandstein the red marl. But when it is once acknowledged, what it is impossible to deny, that their shell limestone (muschelkalk) is not the zechstein, but in reality the second floetz limestone; it then naturally follows that, as every where else, the salt deposit lies under the great mass of that formation, and alternates with every part of it."

"The shell limestone (*muschelkalk*) of Wirtemberg, or of Wurzberg, is in every respect the same as that of the north of Germany, and above it comes the *quadersandstein*, or third floetz sandstone, which surrounds the Jura chain, and lies under it. The most interesting parts of this deposit are the environs of Amberg, where it contains short beds of marly rock, with vegetable impressions (lycopodites), or siliciferous beds, and a kind of coarse tripoli with carpolites. The *lias* lies above it, and alternates with argillaceous and sandy beds; it is a compact marly rock, of a greyish colour, or slaty, with gryphites arcuata, plagiostomata, ammonites, belemnites, mytiloides, reptiles, &c. in short, with all the fossils common to the lias and alumslate of England; so that I would recommend this part of Germany to the study of those English geologists who are inclined to confound the shell limestone (muschelkalk) of Germany with the lias, because the first deposit does not appear to exist, or but very sparingly, in their own country. This formation is also very interesting, from its clay containing masses of brown iron-ore, or hydrate of iron, which are wrought with advantage, and which rarely contain small veins of wavellite, and of oxide of manganese, and are here and there changed by the quantity of marine exuviæ into granular or compact, or even into beautiful crystallized phosphate of iron (Amberg). The well-known nests of compact and

reniform *phosphorite* are also found in a clay subordinate to the lias of Amberg."

"The structure of the secondary formations of the Alps has puzzled many geologists; yet the means of cutting the Gordian knot have been given by Escher, De Buch, Mohs, Lupin, Uttinger, Pantz, Keferstein, &c. The writings of these excellent geologists, together with the judiciously managed travels of Mr. Buckland, have enabled us at last to acquire a distinct view of this part of the alpine regions. It would be quite useless for me to relate my own observations in this place, were I not of an opinion different from that of Prof. Buckland upon the newer deposits of the Alps."

"Upon the *old red sandstone* rests the *great alpine calcareous tract*, which belongs to the zechstein or magnesian limestone; it is in great part a magnesian limestone, which presents some varieties of rocks, one of which is rather compact, another somewhat granular, while another is fetid, and some, particularly those in the upper part of the formation, are porous, or present the structure of the rauchwacke (Eisenertz). In its lower parts there are vast deposits of lead and zinc, in the form of small veins; *bitumen* is found here and there in it; in some places mercury has been collected, which could only come from some bituminous part of this formation, and here and there are found *columns of porphyry*. (Hiedeberg, Geisalp.) This grey, or yellowish, or whitish limestone, forms very high hills of at least 7000 or 9000 feet, and its masses very rarely show any traces of stratification. Petrifactions are exceedingly rare in it. It is the hochgebirgeskalk of Escher and Uttinger, and a part of the alpine limestone of Humboldt, Freisleben, De Buch, &c. It is impossible to confound it with any other limestone deposit, for it has not the slaty structure of the transition limestone, nor the petrifactions of the shell limestone (muschelkalk), and, besides, it lies everywhere under the variegated sandstone and salt-formation. This *last formation* presents, in the Alps, as elsewhere, two masses, an arenaceous and a marly. The first is composed of alternations of greywacke-like micaceous sandstone, seldom very coarse, with marls which are of a greyish, brownish, or yellowish colour; in short, not red like the variegated sandstone of Germany, because in the northern part of the Alps there have been no porphyries, to give them the necessary supply of hydrated oxide of iron. These rocks are placed above, and sometimes also below the marly masses, which consist of alternations of various marls, more or less indurated, and of a brown, reddish-brown, blackish, greyish, or greenish colour: they contain gypsum and rock-salt. Petrifactions are not seen in this formation, but there are many vegetable remains, often of marine plants (Kahlenberg). This formation, which is distinctly stratified in thin layers, lies between the magnesian limestone and the shell limestone (muschelkalk); and, as elsewhere, the upper part of it often alternates with indurated marl or limestone, or even with limestone identical with the shell limestone (muschelkalk), and with flinty concretions. Thus, at Ischel, the marly mass lies between the shell limestone (muschelkalk) and a series of marly and calcareous beds; between Klosternenberg, near Vienna, and Nussdorf, the undulated beds of the deposit contain many limestones, which are here and there traversed by minute ferruginous veins, like the reniform marble of Florence. After this short description, I imagine no one can any longer doubt

the identity of this deposit with the red marl. This formation fills up the valleys of the Alps, and forms only in the eastern part, and in the Carpathians, most extensive ranges of hills, like the Spessart. It is the grès houiller of Beudant, and of my former memoir."—(Memoirs of Wernerian Society, vol. iv. Part I.)

"As this deposit lies upon a very irregular surface, it forms, as elsewhere, many undulations, and affords the first origin of the undulated stratification of the hills of shell limestone (*muschelkalk*), which overlie this formation. The *alpine* shell limestone (*muschelkalk*) is a compact limestone, of a whitish, greyish, yellowish, brownish, and rarely blackish or reddish colour. It contains imbedded flinty concretions, and is traversed by many small veins of calcareous spar, which are generally totally different from those of the transition limestone, and the thin numerous veins of the magnesian limestone, in short, are analogous to those of the shell limestone (muschelkalk). These rocks, which are in some few instances of a particular granular or oolitic structure (roggenstein), afford marbles intermediate between the marbles of the transition limestone, and those of the lias or Jura limestone. They contain many of the same fossils, as the shell limestone (muschelkalk) of the north of Germany, ammonites, modiola socialis, nautili, strombites, turbinites, fragments of echini, madrepores, tubipores, alcyons, &c. They form very high hills, composed of thin beds always stratified, which affords a good test to distinguish this limestone from the magnesian, upon which it often lies in patches or hills. It abounds around the salt district, in Austria, Switzerland, Dauphiné; in short, it is a part of the alpine limestone of authors.

"After this description, I need only add, that I see nothing in it of the character of the lias or Jura limestone, as Mr. Buckland calls this deposit. Its intimate connexion with the salt formation, its situation, its petrifactions, its nature, all show that it is the shell-limestone formation (muschelkalk), so long neglected, and which now seems to occupy so conspicuous a place in nature. It is probable, that even a great part of the limestone lying upon the Macigno, or variegated sandstone of the Middle Appennines, belongs to the shell limestone (muschelkalk), and not to the Jura limestone. Yet, in contradicting in this manner so intelligent an observer as Buckland, I do not, by any means, consider it impossible that some *patches* of the Jura formation may be situated near, or upon the Alps, in some parts; but in Germany I do not know of any facts which show the probability of this statement, and so long as Mr. Buckland is without a clear idea of the shell limestone (muschelkalk), and of its difference from the lias, at least in Germany and France, he will probably hesitate as to the accuracy of my observations. His chief arguments are derived from the petrifactions; but is it not very natural that the same terebratulæ, or some other similar petrifactions, may exist both in the shell-limestone (muschelkalk), and lias? and until he show me in the alpine shell-limestone (muschelkalk), the gryphites, the icthyosauri, the plagiostomata, and show that it is unconnected with the salt deposit, I cannot adopt his ideas, which seem to me inconsistent with nature."

Article XVI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

A new Edition of Berthollet on Dyeing, with Notes and Illustrations. By Dr. A. Ure. 2 Vols. 8vo.

Lectures on the General Structure of the Human Body, and on the Anatomy and Functions of the Skin. 8vo.

A. B. Lambert, Esq. FRS. &c. is preparing a Supplement to his splendid work on the Natural History of Pines. With Engravings. Folio.

JUST PUBLISHED.

On the Stratification of Alluvial Deposits, and the Crystallization of calcareous Stalactites. In a Letter to Dr. John Macculloch. By H. R. Oswald. 8vo. 1*s.* 6*d.*

A Treatise on the Medicinal Leach, including its Medical Natural History, with a full Account of its very singular Anatomical Structure, &c. By J. R. Johnson, MD. FRS. FLS. 8vo. 8*s.*

A Guide to the Giant's Causeway and North-east Coast of the County of Antrim, containing an Account of the Geological Structure of Basaltic Stratification. By the Rev. G. N. Wright. With Map and Plates. Royal 18mo. 6*s.*

Mr. G. B. Sowerby's Genera of Recent and Fossils Shells: Nos. 16, 17, and 18; containing the following Genera:—Unio, 2 Plates; Conus, 2 Plates; Hyria; Calceola; Cypræa, 2 Plates; Anodon, 2 Plates; Lima; Nucula; Anomia; Ricinula; Corbula; Pyrgoma; Creusia; Trigonia.

Mémoires de la Société d'Histoire Naturelle de Paris. Tome Premier. 1re Partie. Paris, Boudoin Frères, 1823. 4to. 15*s.*

Article XVII.

NEW PATENTS.

T. W. Stansfield, Leeds, worsted manufacturer; H. Briggs, Luddendenfoot, Halifax, worsted manufacturer; W. Richard, Leeds, engineer; and W. Barraclaugh, Burley, Leeds, worsted manufacturer; for their improvements in the construction of looms for weaving fabrics composed wholly, or in part, of woollen, worsted, cotton, linen, silk, or other materials, and in the machinery and implements for, and methods of working the same.—July 5.

G. Clymer, Finsbury-street, Finsbury-square, mechanic, for certain improvements in agricultural ploughs.—July 5.

J. Fisher, of Great Bridge, Westbromwich, Staffordshire, ironfounder, and J. Horton, the younger, of the same place, manufacturers of steam boilers, for improvements in the construction of boilers for steam engines, and other purposes where steam is required.—July 8.

S. Fairbanks, of the United States of America, but now residing in

Norfolk-street, Strand, merchant, for certain improvements in the construction of locks and other fastenings.—July 10.

J. L. Bradbury, Manchester, calico-printer, for improvements in the art of printing, painting, or staining silk cottons, woollen, and other cloths, and paper, parchment, vellum, leather, and other substances, by means of blocks or surface painting.—July 15.

B. Gill, Birmingham, merchant, for certain improvements in the construction of saws, cleavers, straw-knives, and all kinds of implements that require or admit of metallic backs.—July 15.

Sir I. Coffin, Bart. Pall-mall, Middlesex, for a certain method or methods of catching or taking mackarel and other fish.—July 15.

W. Palmer, Lothbury, paper-hanger, for his improvements in machinery applicable to printing on calico, or other woven fabrics composed wholly or in part of cotton, linen, wool, or silk.—July 5.

W. H. Horocks, Portwood within Brimington, Cheshire, cotton manufacturer, for certain methods applicable to preparing, cleaning, dressing, and beaming silk warps, and also applicable to beaming other warps.—July 24.

R. Gill, Barrowdown, Rutlandshire, fellmonger and parchment manufacturer, for his method of preparing, dressing, and dyeing sheepskins and lambskins with the wool on for rugs, carriages, rooms, and other purposes.—July 24.

W. Jeaks, Great Russell-street, Bloomsbury, for his apparatus for regulating the supply of water in steam-boilers, and other vessels for containing water or other liquids.—July 24.

W. Davis, Bourne, Gloucestershire, and Leeds, Yorkshire, engineer, for certain improvements in machinery for shearing and dressing woollen and other cloths.—July 24.

H. Smart, Berner's-street, piano-forte manufacturer, for certain improvements in the construction of piano-fortes.—July 24.

M. Turner, and L. Angell, both of Whitehaven, soap-boilers, for their process to be used in the bleaching of linen, or cotton yarn, or cloth.—July 24.

J. Jackson, Nottingham, gun-maker, for certain improvements in the locks used for the discharge of guns and other fire-arms upon the detonating principle.—July 29.

J. Bower, Hunslet, Leeds, oil of vitriol manufacturer, and J. Bland, Hunslet, Leeds, steam-engine manufacturer, for their improvements in such steam-engines as condense out of the cylinder, by which improvement the air-pump is rendered unnecessary.—July 31.

J. Bainbridge, Bread-street, Cheapside, merchant, for certain improvements upon machines for cutting, cropping, or shearing wool, or fur from skins; also for cropping or shearing woollen, silk, cotton, or other cloths and velvets, and also for the purpose of shaving pelts or skins.—July 31.

L. J. Pouchee, King-street, Covent-garden, type-founder, for certain apparatus to be employed in the casting of metal types.—Aug. 5.

R. Dickinson, Park-street, Southwark, for his improvement in addition to the shoeing or stopping and treatment of horses' feet.—Aug. 5.

J. Barron, Wells-street, St. Mary-le-bone, venetian-blind manufacturer, and J. Wilson, Welbeck-street, Mary-le-bone, upholsterer, for certain improvements in the construction and manufacturing of window blinds.—Aug. 11.

ARTICLE XVIII.

METEOROLOGICAL TABLE.

1823.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
7th Mon.								
July 1	Var.	30·10	30·01	74	52	—	25	
2	N W	30·14	30·10	75	47	—	—	
3	N	30·17	30·12	78	49	—	—	
4	E	30·12	30·04	65	49	—	10	
5	W	30·04	29·85	74	60	—		
6	W	29·85	29·84	71	50	—		
7	W	29·84	29·82	68	49	—	20	
8	N W	30·03	29·82	66	44	·85	20	
9	N W	30·15	30·03	74	47	—		
10	S W	30·15	29·85	74	49	—		
11	S W	29·85	29·82	74	56	—		
12	S W	29·85	29·82	70	58	·75		
13	S	29·82	29·78	73	54	—		
14	S W	29·89	29·82	72	50	—	16	
15	S W	29·82	29·75	70	50	—	20	
16	S W	29·95	29·73	66	48	—	23	
17	N W	29·95	29·91	66	56	—	05	
18	SE	29·99	29·91	66	54	—	06	
19	S W	30·07	29·99	71	60	·76	04	
20	W	30·07	29·80	78	57	—		
21	S W	29·91	29·80	71	49	—	04	
22	N W	29·78	29·57	71	54	—	—	
23	S W	29·76	29·62	71	53	—	21	
24	N W	29·92	29·76	68	46	—	—	
25	S W	29·92	29·75	68	50	·84	15	
26	N W	29·92	29·76	65	50	—	40	
27	N W	29·96	29·92	68	53	—	08	
28	S W	29·96	29·91	68	53	—	04	
29	S W	29·91	29·88	69	47	—		
30	W	29·99	29·91	70	54	—	02	
31	W	30·18	29·99	74	50	·62		
		30·18	29·57	78	44	3·82	2·43	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Seventh Month.—1. Showery. 2. Cloudy, and fine. 3. Fine. 4. Showery. 5. Cloudy. 6. Fine: occasional clouds. 7. Showery. 8. A very heavy shower of rain about half-past three, p. m. attended with thunder. 9, 10. Fine. 11. Fine: cloudy at intervals. 12. Cloudy and fine. 13. Rainy. 14. Cloudy: slight showers. 15. Fine morning: showery afternoon. 16. Rainy. 17. Fine. 18, 19. Showery. 20. Fine. 21. Showers. 22. Cloudy. 23. Showery day: heavy rain with thunder about six, p. m.: some thunder showers afterwards, with lightning. 24. Cloudy. 25. Showery. 26. Rainy. 27. Cloudy and fine. 28. Cloudy. 29. Overcast. 30, 31. Fine.

RESULTS.

Winds: N, 1; E, 1; SE, 1; S, 1; SW, 12; W, 6; NW, 8; Var. 1.

Barometer: Mean height

For the month.................................... 29·915 inches.

For the lunar period, ending the 1st.................. 30·004

For the lunar period, ending the 30th................. 29·910

For 13 days, ending the 12th (moon north) .... 29·980

For 14 days, ending the 26th (moon south) .... 29·847

Thermometer: Mean height

For the month.................................... 62·677°

For the lunar period, ending the 1st.................. 57·034

For the lunar period, ending the 30th................. 62·666

For 30 days, the sun in Cancer .... 59·983

Evaporation.. 3·82 in.

Rain. .... 2·43

Laboratory, Stratford, Eighth Month, 22, 1823. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1823.

Article I.

Some Account of a scarce and curious Alchemical Work, by Michael Maier. By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

As you did not think the account of Biringuccio's Pirotechnia unworthy of admission into your journal, you may, perhaps, be disposed, on the same principle, to spare a few pages to the analysis of another early work, in its own line not less curious and interesting. For there is always, unless I am much mistaken, an interest, and that a strong one, in tracing the history of arts and science even where it exhibits most strikingly the aberration and misuse of human intellect and industry.

Believe me, dear Sir, most sincerely yours,

J. J. Conybeare.

Symbola Aureæ Mensæ Duodecim Nationum. Authore Michaele Maiero Com. Imp. Cons. Nob. MD. &c. Francofurti, 1617. Small 4to. pp. 621.

Maier is termed by Beckman (unless my recollection be incorrect), the most learned alchemist of his age; and of all the alchemical works into which I have been occasionally led to search, this appears the best calculated to afford the curious reader an insight into the history of that art, and of the arguments by which it was usually attacked and defended. It has

the additional merit of being more intelligible and more entertaining than most books of the same class.

According to the taste of his day, Maier has thrown his defence of alchemy into the form of an allegorical narrative. The virgin Chemia having been grossly and falsely slandered by some adversary whom he names Pyrgopolynices, summons to her defence twelve worthies of as many countries, who assemble in solemn council round the Golden or Philosophic Table. In agreement with the number of these sages, the work is divided into twelve books or parts, each constructed pretty much upon the same plan. In each, an account is first given of the hero who acts as its Coryphæus; this is followed by brief notices of such among his countrymen as have been eminent in the same mysterious art; and usually by some desultory remarks as to the natural and other peculiarities of the country which produced them. Lastly, Pyrgopolynices is introduced making a *syllogistic* attack upon some one or more leading points of alchemical doctrine, which is readily answered by the aforesaid Coryphæus with all due etiquette of *major, minor, &c.*

The first character thus brought upon the stage is Hermes Trismegistus, whose pretensions to this eminence can hardly be unknown to any of your chemical readers. Maier determines seriously that Hermes lived 2000 years before the Christian era, appears to acquiesce in the spuriousness of such decidedly alchemical works as passed under his name, and rests his claim to the title of Prorex Chemiæ on a forced interpretation of some passages in the Pimander and Asclepius, theosophical tracts fathered upon Hermes by the forgers of the Alexandrian school, and in two short tracts, the Smaragdine Table, and the Tractatus 7 Capitulorum, of more dubious origin and signification. Under this view, he of course regards the mythology and hieroglyphics of Egypt as concealing the arcana of the *Hermetic* art. This opinion, however, is reasonable in comparison with one which he states to have been entertained by some of his contemporaries, "that the whole Scripture, both of the Old and New Testament, is nothing more than a body of chemical allegories." This Maier, who does not appear to have been deficient in piety, deservedly reprobates. The earliest authority which, with all his research and erudition, he can produce for the chemical learning of the Egyptians, is the assertion of Paulus Diaconus (a writer of the eighth century), that Diocletian burnt the library of Alexandria in order to prevent the Egyptians from becoming learned in the art of producing at will those precious metals which might be employed as the *sinews of war* against himself. He misquotes Orosius as an evidence to the same purpose. It is needless to say, that as far as chemistry is concerned, the story is evidently a fiction. He attempts to press Tacitus into the same service, on the presumption that the

Phœnix mentioned Ann. l. 6, is an allegorical representation of the Philosopher's Stone.

To Hermes, Maier gives for assessors nearly all the early Egyptian kings, Adfar, the Alexandrian, and Calid, the Saracen. Of these, he affirms that the immensity of their works, and the hieroglyphic remains of Egypt, prove more plainly than the sun at mid-day that they were great alchemists. Among those who received immediately from Egypt the doctrine of the adepts, were the Phœnicians (Cadmus was an alchemist, and the Hydra the *dragon* of his art), the Colchians (witness the golden fleece), the Phrygians (he seems to insinuate that the war of Troy is a chemical allegory), and the Eumolpidæ of Eleusis. But, he proceeds, it is asked, "If chemistry be of such antiquity, and if its secrets have been in the possession of so many persons from the earliest ages, whence is it that they yet remain secrets." For this natural question, he has no better answer than that of all his brethren, "that they who had *the gift* were under a moral obligation to perpetuate their knowledge only under the veil of symbols and allegories, penetrable by those alone whom heaven should see worthy of such a privilege." In the train of the Egyptians follow the Gymnosophists of Æthiophia, the Magi of Persia, and the Bramins of India. He quotes from the life of Apollonius, a passage, which renders it not altogether improbable that in the age of Philostratus, somewhat of alchemical quackery had already begun to mix itself with the speculations of the mystic and Theurgic philosophy. Jarchas, the Bramin, conversed, he says, with Apollonius, among other things, concerning the *water of gold.*

Pyrgopolynices now begins his attack. "No species," he asserts, "is changeable into another species. But gold, copper, lead, &c. are species *per se,* ergo, they are not commutable *inter se.* The answer which one would anticipate at the present day is, that the determination of the species must be matter of experiment, and that if copper, e. g. be an impure or adulterated gold, it is not a species *per se.* The answer of Hermes, however, is, that one species does actually pass into another, e. g. a specific egg into a specific chicken, a seed into a plant, &c. But his strong proof (or battering ram, as he terms it), is the evidence of all persons concerned in metallurgy in favour of the natural transmutation of metallic species.

Chap. II. *Hebrews.*—This class is led by Miriam, or Maria, whom Maier believes to have been the same with the sister of Moses, chiefly because Moses himself was skilled in the arts of the Egyptians, and because operations requiring a certain degree of chemical knowledge are mentioned in the books of Exodus and Leviticus. The writings ascribed to this Miriam are next quoted as alluding to the Vas Hermetis (the same, according to Maier, with the fiery cup of the Bramins mentioned by Philostratus.) "*Vas,*" says Miriam, "*quod Stoici occultaverunt.*"

I have not the means of referring to the original tract, but from this mention of the Stoics, should apprehend it to be, if not among the earliest alchemical forgeries, subsequent to the revival of literature.* To what particular notion of the Stoics the author refers, I am not aware; the passage, however, if the tract be of any antiquity, is a curious one. Other Hebrews are enumerated as eminent in the art, among whom Solomon, as might be supposed, is not overlooked; somewhat less plausible is the insertion (on the authority of Avicenna, Vincent of Beauvais, and the Peræ Ecclesiasticæ) of St. John the Evangelist. The notion seems to have had its rise in the misconception of a legend which represents St. John as having converted stones into gems, and wood into gold, for some eleemosynary purpose. This section is, for the most part, very dull and uninteresting. I will add nothing more, therefore, than a specimen of the arguments with which it concludes. "*To that which is perfect* (says the adversary), *nothing can be added; but the inferior metals, as lead, &c. are perfect, therefore nothing can be added to them.*" It is answered that that which is *naturally* perfect in its kind may be yet further perfected by art, as corn which is perfect in se is yet further perfected by being made into bread, &c.

Book III. *Greeks.*—These are headed by Democritus the Abderite, for whose existence, philosophy, and merriment, sufficient authority is given; for his alchemy, that of Psellus and Picus Mirandulæ. Maier hints that the atomic theory might still have its supporters, if the Aristotelians did not cry it down, but objects strongly to the notion of a plurality of worlds. The catalogue of Greek *alchemists* includes Orpheus, Homer, the authors of the mysteries, and even of the Olympic and other games, Pythagoras, and nearly all the Greek physiologists; among the rest, Euclid, and *Seneca*, *Hamech*, and *Abugazal, the master of Plato*. Apollonius of Tyana is made, with somewhat more of plausibility, to occupy a prominent station among these gentry. Into the probable sources of the extraordinary hallucination which would convert nearly the whole learning of Grecian antiquity into a mere vehicle for the dreams of alchemy, I shall endeavour to inquire shortly. Here then I will add only a further specimen of alchemical dialectics. P. "From two elementary substances (*entibus per se*) one ens per se cannot be made. But the alchemist who affirms that *gold* (an ens per se), may be made by the union of lead, and *the tincture* assumes this. This assertion is, therefore, false." Answer. "We constantly see "unum quid" made "ex duobus entibus," as bread of

* It is probably an early Greek forgery. G. Syncellus (A. D. 780), mentions one Maria, a Hebrew, as contemporary with Democritus of Abdera, and having written in language purposely obscure on subjects of the same kind, namely, *gold*, *silver*, *stones*, and *purple*. "Miriam and the Jewish writings," are also referred to in a Greek MS. entitled "The Sacred Art," in the Royal Library of France."—(Fabricii Cod. Apocryph. Vet. Test. vol. i. p. 869.)

flour and leaven, cheese of milk and rennet, &c. and one whole house of its several parts." Nothing (he concludes), save incredulity or ignorance, can see a difficulty here.

Book IV. *Romans.*—The earliest alchemical authority our author is able to find among the Romans is one Morienus, whom he states to have lived about A. D. 800. He argues, however, that the Romans must have been acquainted with the Hermetic art from their knowledge of the mythology and philosophy of the Greeks, and from the extent of their public revenues. He employs much erudition to little purpose, and quotes as alchemical the well-known enigmatic epitaph *Ælia, Laelia, Crispis,* said to have been found with a perpetual lamp, and a second in which occur the following lines:

> Hic elementa brevi clausit digesta labore,
> Vase sub hoc modico Maximus Olybius.

Both are probably forgeries of the 15th century. Virgil wrote alchemy. The golden bough of the sibyl, and indeed the whole descent of Æneas to the shades, is an allegory of this kind: he wisely omits all notice of the bard's "*porta emittet eburnâ.*" He notices the tradition that Virgil was a necromancer, a fancy at least as old as the 12th century. This section concludes like the former with a logical disputation. The arguments, as we have seen, are either mere verbal *equivoques,* or barefaced assertions, that the metals have been decomposed and recomposed by sundry alchemical worthies.

Thus Maier concludes his review of the supposed Chrysopoetic science of the earlier and classical ages. It is unnecessary to add, that the whole can be considered at the present day only as a tissue of fiction, or at best of gratuitous assumption and gross misconception.

Among all that his labour and erudition have brought together, there is not a single real authority (if we except the very obscure passage in Philostratus) on which we can ground even a suspicion that alchemy was studied or heard of at any time previous to the utter declension of art and literature in the eighth and ninth centuries.

Yet that Maier and many others did sincerely believe much at least of what they affirmed concerning the history, as well as the reality of their art, can scarcely be doubted, nor is it, perhaps, difficult to trace the causes which tended to produce and to confirm these hallucinations.

The 16th century was no more the age of critical than of philosophical accuracy, and forgeries of all kinds were, therefore, received with less of question and examination. Add to this, that the mind of the adept already habituated to a symbolical language, chiefly borrowed from the heathen mythology, was the more easily led to assume, that the whole of that mythology was little more than the *involucrum* of chemical science. It will be

remembered too that the learned of Maier's age almost universally agreed in attributing to the varied and absurd fables of classical superstition an allegorical meaning of one kind or other; much of it had long since been regarded as shadowing out the phænomena and constitution of the material universe.* Fictions which were, or were held to be, thus symbolical of the great and universal operations of nature, might easily, either by transfer or misconstruction, be applied to the more restricted but yet analogous processes of the laboratory. Generation, mixture, separation, dissolution, and reproduction, formed equally the study, and were equally in the mouth, of the philosopher who speculated on *generals*, and of the artist whose labours were confined to the detail of experiment.

Nor does it appear altogether an absurd or untenable hypothesis, that the whole fabric of alchemical delusion had its origin in the misinterpretation of those cosmological works which were popular in the declining age of classical literature. The Alexandrian and other schools which mingled much of oriental philosophy with the systems, real or pretended, of Pythagoras and Plato, seem to have abounded in this lore, and to have expressed it not unfrequently in a figurative or symbolical manner. They produced also many forgeries attributed usually to authors of a high antiquity, and occasionally designed, perhaps, to prop the failing cause of heathenism. These, in process of time, would become unintelligible, and a new set of impostors or fanatics† would intentionally or credulously distort their enigmatical contents, to the illustration of theories equally visionary, but better calculated to attract and dazzle an ignorant and barbarous age. We know at least that the *Sealed or Hermetic Vase* was of old considered as the symbol of the material universe, ever full, but never overflowing. The *Mundane Egg* was the same; and the serpent with the tail in his mouth figured the eternity of that universe (a well-known dogma of the pseudo-Pythagorean school), while fire was the type of the vivifying principle which pervades and preserves the whole. These are all common to the schools both of cosmogony and of alchemy,‡

* Thus in the well-known lines of Virgil.—

> Quum Pater omnipotens facundis imbribus æther,
> Conjugis in lætæ gremium descendit.

There are traces of this mode of interpretation in the remains of a much earlier poet, the philosophic Empedocles; the Stoics and the Platonists (at least the later Platonists) were also much given to it.

† There is in truth little to choose between such writers as Philostratus or Iamblichus, and R. Lully or Ripley.

‡ For the former, I would refer the scholar to the learned though sometimes fanciful essays of Creuzer, entitled, "Dionysius, &c." (Heidelberg, 1809); for the latter, to the alchemical hieroglyphics engraved in Barchusen's Chemia (Leyden, 1718). It may be added that Beckman and Bergman both quote from Origen against Celsus, an account of a Persian temple, in which the different planetary spheres were represented by different metals. It seems probable that the metals were employed in talismans, &c. as symbolical of the planets, long before the names of the planets were used to designate the metals.

and more resemblances might, I suspect, be traced by any one who had the inclination and opportunities to examine the earlier forgeries termed alchemical, those especially which are extant, or were originally written, in the Greek language.

(To be continued.)

ARTICLE II.

On the Changes which have taken place in the Declination of some of the principal fixed Stars. By John Pond, Esq. Astronomer Royal, FRS. Read April 18, 1822.*

THE mural circle having in September last been put into complete repair, and declared by Mr. Troughton to be in as perfect a state as when first erected, I resumed my examination of the principal fixed stars which form the Greenwich Catalogue. In the course of a very short time, I found that several anomalies, which had previously given me much perplexity, still subsisted: some of these were of such a nature as to lead to a suspicion that a change might possibly have taken place in the figure of the instrument; on the other hand, there were circumstances, that strongly militated against such a supposition.

Several of the stars in which the supposed discordance appeared the greatest, passed over almost the same divisions with others, in which no such discordance could be perceived. Moreover, in examining these discordances in different points of view (that is, both with respect to their right ascensions and polar distances) I fancied I perceived something like a general law, that was quite incompatible with any possible hypothesis of error in the instrument.

On a point of this importance, I clearly saw the necessity of devising some new method of observation which might decide with certainty, that which otherwise would become an endless subject of doubt and conjecture.

I had often attempted to observe the altitudes of stars by means of an artificial horizon of quicksilver, or other fluid, but had abandoned the attempt from the difficulty of protecting it from the wind, and from the number of observations I lost in fruitless experiments. To this method I had again recourse; and by means of wooden boxes of different sizes and figures, according to the different altitudes of the stars, I have sufficiently accomplished my purpose. A very few observations were sufficient to convince me that the instrument was in every respect

* From the Philosophical Transactions for 1823, Part I.

perfect, and that I might repose the greatest confidence in every result it gave.

Several stars, and particularly those most discordant, I have observed by this new method, and find their places, without any exception, to agree within a fraction of a second, with those determined by direct measurement from the pole.

Presuming that the observations* which accompany this paper will remove every shadow of a doubt as to the accuracy of the instrument, I shall now proceed to state, in as few words as possible, the nature of the changes which appear to me to have taken place since the year 1812.

If Bradley's catalogue of stars for the year 1756, be compared with the Greenwich catalogue for 1813, it will be possible to deduce the annual variation for each star for the mean period, or for the year 1784, on the supposition of uniformity in the proper motion of each star; then allowing for the change of precession for each star, a catalogue may be computed for any distant period; as for example, the present year 1822. Suppose such a catalogue computed, which I have named a predicted catalogue; then, if this be compared with the observed catalogue for the same year, the following differences will be found to subsist between them.

The general tendency of all the stars will be to appear to the south of their predicted places, and this tendency seems to be greater in southern than in northern stars; if any star be found north of its predicted place, it will always be a star north of the zenith, and the quantity of its motion extremely small. There may be observed a much greater tendency to southern motion in some parts of the heavens than in opposite or distant parts as to right ascension, and in much the greater portion of the heavens the southern motion seems to prevail. A southern star, as Sirius, situated in that part of the heavens most favourable for southern motion, will be found more to the south of its predicted place than Antares, situated in the part least favourable for southern motion, though it is itself more southward.

Several stars have moved more from their predicted places than other neighbouring stars; when this happens, the motion is always southward; I have yet met with no exception to this rule; not a single star can be found having an *extra* tendency to northern motion; and indeed the northern motion in any star is so very small, that it would never have excited attention.

A very great deviation will be found in three very bright stars, Capella, Procyon, and Sirius: the proper motion of each of these is southward; it therefore follows that these proper motions are accelerated. The proper motion of Arcturus is very great, and likewise southward. It is situated in that part of the

* These observations are given, in the Transactions, in a copious appendix of tables to this and the two succeeding papers, which, on account of its length, we are compelled to omit.—*Edit.*

heavens where the southern tendency is least discernible, and is nearly quiescent; its proper motion in polar distance may, therefore, be considered as uniform. There is a circumstance that deserves notice, though it may be merely accidental: the stars in the Greenwich catalogue, whose proper motions are south, nearly equal in number those that are north, yet the *quantity* of southern proper motion exceeds the northern in the proportion of four to one.

I shall at present offer no conjecture on the cause of these deviations, but endeavour, by continued observations, more accurately to ascertain the law which they follow. Should the weather prove favourable for observation, I hope before the Society separate for the summer, to be able to give greater accuracy to the numbers here subjoined. Indeed I should not have made so early a communication on the subject, but as the Greenwich observations of 1820 are about to be published, they might without this explanation have appeared erroneous; for I find that during that year the instrument was rather defective from general unsteadiness, than from any perceptible deviation of the telescope. It was not till after the month of Feb. 1821, that the instrument got completely out of repair. It must however be admitted, that the observations of that year ought not to be employed in the determination of such small quantities as form the subject of the present communication.

Horizontal Point of the Circle as found by different Stars observed by direct Vision and Reflection from 11*th to* 23*d March,* 1822.

h Urs. Maj.	123° 30′ 29·55″
v	28·95
m	29·75
β	29·45
α	29·50
o	29·05
Castor	29·86
Capella	29·55
Pollux	29·95
β Aurigæ	29·35
Mean of 10	29·54
Sirius	29·47

There being no perceptible difference in the results obtained near the zenith and near the horizon, it may be concluded that the instrument has no deviation, either from flexion of the telescope or change of figure.

Article III.

Appendix to the preceding Paper on the Changes which appear to have taken place in the Declination of some of the fixed Stars. By J. Pond, Esq. Astron. Royal, FRS. Read Nov. 14, 1822.*

The observations which have been made during the last summer, confirm in a very decided manner the results which formed the subject of my last communication; in which I laid before the Society the nature of the differences that exist between the computed places of the principal Stars of the Greenwich Catalogue, and those deduced from actual observation. It is not my present intention to offer any explanation of the cause of these phænomena, although many obvious conjectures present themselves, the value of which it will require perhaps many years to determine. It is now my principal object to consider the force of that explanation of the differences in question, which will most readily occur to every astronomer, namely, that the whole may arise either from error committed by the observer, or from defect in the instruments of observation: this objection being the more weighty from the circumstance, that the observations of three distant periods are employed, and that an error in those of either period (but particularly of the two latter) would materially affect the result now under consideration.

I believe that every person, in proportion to his experience in the use of astronomical instruments (even of the most unexceptionable construction), will be cautious in admitting the accuracy of any results, with whatever care the observations may have been made, which appear to militate against any received theory of astronomy; and I shall have occasion myself to show, from the great discordances between instruments of the highest reputation, that this distrust is but too well founded. More particularly ought our suspicion to be excited, when such anomalies are found to exist, as bear some direct proportion to the zenith distances of the stars observed. In all such cases we should never hesitate, I think, to ascribe the anomalies to defective observation. If therefore in the present instance, any part of the discordances in question can be shown to depend on polar or zenith distances, I shall willingly admit, as to such part of them at least, that they are no otherwise of importance, than as affording data for leading to the detection of some hitherto undiscovered errors. The anomalies, however, that have led me on to this inquiry, and to which alone I attach any importance, are found to depend rather on the right ascensions, than on the

* From the Philosophical Transactions for 1823, Part I.

declinations of the stars. Accordingly I found, while collecting observations to form a catalogue for the present period, that I could more nearly predict the deviation of a star from its computed place, by knowing its right ascension, than its declination. Now it is not easy to conceive in what way the error of an instrument for measuring declination, fixed in the meridian, can be occasioned by any circumstance depending on the right ascension of a star to be observed.

The general nature of the deviation of the stars from their computed places will be best understood from the annexed tables;* in one of which the principal Stars of the Greenwich Catalogue are arranged according to north polar distance, and in the other, in the order of their right ascensions.

From these tables, it will appear, according to my statement in the former part of this paper, that the general tendency of the deviation is towards the south: that in about one-third part of the heavens in right ascension this southern tendency is very inconsiderable, and would hardly have excited attention: for in this part, stars between the zenith and the pole, appear a very small quantity to the northward; whereas in the remaining, and most considerable portion of the heavens, every star appears to be a considerable quantity to the south of its computed place; and with few exceptions, the more southward stars have a greater tendency to deviation than the northern ones.

If we select from the preceding tables those stars which were least frequently observed, at one or all of the three periods, we shall find that they all tend to confirm the foregoing general results; though they must be regarded as doing so, rather by their united effect, than by their weight of evidence when considered singly. Stars that have been but seldom observed, give results considerably affected by accidental error of observation; which error is quite of a different nature from that produced by permanent defect in the instrument, and which repetition of observation has no tendency to remove.

If the deviations of those stars that have been imperfectly observed, were attributable either to error of observation, or defect in the instruments, the deviation would either follow no law at all, or some law depending upon zenith distance: but the facts we have seen to be at variance with either of these hypotheses. Not however to rest satisfied with these considerations drawn from the general tendency of all the stars without exception, let us select some striking examples of deviation, in particular groups of stars, on which we might be satisfied to rest the issue of this question. Of these groups I have marked *five*, in the table of stars arranged according to north-polar distance, each of which we will take the pains to consider more attentively.

1. There are six stars in my Catalogue north of γ Draconis,

* These are necessarily omitted in this work: see note to p. 248.

of which three are found to the north, and three to the south of their computed places. These inequalities may appear at first sight to be wholly accidental; but if we pay attention to the right ascension, we shall find that the three which appear to the northward, are situated in that part of the heavens as to right ascension where the southern deviation is the least perceptible, and that the three which appear to the southward, are in that part as to right ascension where the southern deviation is the greatest. But of these six stars there are two, α Cassiopeiæ, and γ Ursæ Majoris, which deserve further consideration. These two stars are within less than one degree of each other in polar distance, and consequently pass over the meridian at nearly the same altitude. The observations of Bradley on the stars north of the zenith are not so numerous as could be wished; but each of the two stars in question was observed by him about five times towards the year 1753; that is 60 years from the date of my catalogue of 1813. I have carefully recomputed the predicted places of these stars, and I find α Cassiopeiæ not less than 1·5″ to the south of its predicted place, and γ Ursæ Majoris half a second to the north. Now I am quite at a loss to conceive how this difference in so small an arc can arise from error of observation, and I can only attribute it to that cause, whatever it may be, which seems so generally to depend not on the polar distance, but on the right ascension of the star.

2. The second group which I shall consider, contains the stars α Arietis, Arcturus, and Aldebaran, comprehended within an arc of about six degrees and a half. Of these three, Arcturus alone has yet been observed by reflection; but from the present very perfect state of the Greenwich circle, which the method of reflection has enabled me to ascertain, it cannot be doubted that the places of the two other stars are well determined.* In Arcturus the southern deviation is nearly insensible, but in the two other stars it is very considerable, being in each not less than 1·5″. Now these three stars, but particularly the two latter, are among those that have been most assiduously observed by Bradley and myself, at each of the three periods. Let us suppose then, if it be possible, that the whole of these deviations arise from error of observation; or, in other words, that no systematic deviation has really taken place in the stars, but that their proper motions are uniform. Then we must admit that the mural quadrant and the mural circle have at each period given the polar distance of Arcturus correct, or at least subject to the same constant error; and as this star has been observed at each period, at all times of the day, and at all seasons of the year, the observations may be considered as perfectly exempt from accidental error. It will I believe be readily conceded that both instruments are so far perfect, that if the error be either nothing, or a given quantity at one point of the arc, the errors

* This has been confirmed by subsequent observation.

must be very nearly indeed the same within a moderate distance, as within 15 degrees, for instance, of that point. Upon this supposition, how can we possibly reconcile the great errors that must have been committed in stars, adjacent as to polar distance, but of opposite right ascensions? I do not wish to press these remarks, in order to obtain greater confidence than they deserve, for observations which can never be regarded with too much suspicion; but the arguments I have used appear to me to follow logically from the data before us, and strongly to indicate the probability that some cause purely astronomical has, at least, some share in producing these unexpected deviations.

3. The third group, α Herculis, α Pegasi, and Regulus, is still more remarkable, being comprehended within two degrees of declination, and two of the stars, α Herculis and α Pegasi,* being within half a degree of each other. In this group α Pegasi is at least 3″ south of its predicted place, whereas the other two stars have not deviated much more than 0·5″ to the south.

4. α Orionis, α Serpentis, and Procyon, furnish an example equally striking, they being within less than 2° of declination from each other; α Serpentis is exactly in its predicted place, while α Orionis and Procyon are each of them at least 2″ to the south.

5. Rigel, Spica Virginis, and Sirius, are not contained within so short an arc as the former groups, nor are their places so well determined, on account of their proximity to the horizon; but they afford another instance of the inequality of southern deviation, in stars having nearly the same polar distance, but opposite right ascensions.

But leaving the considerations suggested by these groups of stars, let us examine more minutely the different hypotheses that may be formed on the supposition, that the whole of these deviations depends on error of observation caused by some defect in the instruments employed: this investigation becomes the more necessary, as it does not appear that Dr. Brinkley, with his instrument at Dublin, has met with similar discordances. Admitting the accuracy of the observations of Bradley to form the ground-work of this inquiry, there are then two distinct hypotheses, that may be formed by those, who are inclined to maintain, that the proper motions of the stars are uniform; and that the discordances in question have their source, not in any astronomical cause, but in some erroneous system of observation. Of the observations from which the catalogues of 1813 and of the present year have been computed, we may suppose the one or the other to be erroneous. Let us consider the consequences of each hypothesis.

Let us first suppose the error to be in the observations of 1813.

* The lunar nutation of α Pegasi was nearly a minimum at each period.

Then the observations of 1756 and 1822 being supposed perfect, a catalogue for the year 1813 may be computed by interpolation; such a catalogue is annexed, and this (assumed to be correct), compared with the observed catalogue of 1813, will show the errors of observations at that period. On this assumption the Greenwich circle must, in 1813, have been in a very defective state; and admitting the instrument to be now perfect, this can be only attributed to the insufficiency of the braces which then connected the telescope to the circle; for this is the only difference between the instrument in its former and in its present state. The natural tendency of any such defect would be, I think, continually to increase, and to give results every year more and more distant from the truth: but this is contrary to the known history of the Greenwich observations, which I have found gradually for some time past approaching to those results which are obtained at the present day, and which, according to our present hypothesis, are supposed to be nearly perfect. If the catalogue of 1813 were really so erroneous, as our present hypothesis would compel us to regard it, then it would appear that Dr. Brinkley's catalogue for the same period must have been still more erroneous, as may be seen by inspection of the annexed tables. Now admitting for a moment that there were at that time certain imperfections in the Greenwich and Dublin instruments, no person will believe them to have been so imperfect as our present hypothesis would tend to represent them.

Let us now examine the second hypothesis, which presumes the catalogue of 1813 to have been perfect, and consider what confidence is due to the Greenwich observations of the present day. This investigation is to be regarded as important, not merely with a view to the discussion of the nature of the discordances in question, but also from the circumstance, that instruments of well-known celebrity are represented as giving very different results; for which reason I shall be excused for entering into considerable details on this particular question. As the principal reliance I place on the accuracy of the present catalogue, and on the superiority of the Greenwich circle over all other instruments, with the history of which I am acquainted, is derived from the coincidence of the results obtained by the two independent methods; the one of direct measurement of polar distance, the other of observing the angular distance of the direct and reflected image of the stars, it becomes of some importance to consider in what way this coincidence is a proof of the accuracy of either. The source of error the most to be dreaded in every instrument whatever, quadrant or circle, is that which will be caused by the flexure of the materials of which the instrument is made. It is impossible in theory that any instrument can be wholly free from this defect. In the Greenwich circle the number of microscopes placed round its circumference have an obvious tendency to diminish this error, though

they cannot annihilate it; but they have no tendency whatever to diminish the error arising from the flexure of the telescope attached to the circle.

The effect of flexure in any circle will be, in the first instance, to give an erroneous distance from the pole to the zenith: in instruments that turn in azimuth, of the usual construction, the error thus occasioned will be applied to every star under the form of co-latitude, and a star south of the zenith, will be moreover affected by the probably opposite flexure due to that point of the instrument on which the star is observed. This in stars near the equator, or a little to the northward of it, will in our latitude give an error in polar distance, amounting to about double the error committed in determining the co-latitude. On the contrary, the polar distances of stars north of the zenith, being affected only by the difference of two flexures, will be more accurately determined as they approach nearer to the pole, where the errors will wholly vanish. Now, though in the usual mode of employing the Greenwich circle, viz. in measuring directly polar distance, the co-latitude does not become an object of enquiry, yet any flexure of the circle will produce a system of errors of the same nature as those above pointed out. In instruments, like that of Dublin, which turn in azimuth, and with which the observer has to find the place of all the stars by measuring the double of their zenith distances, if he does not find the same zenith point with different stars (provided the instrument be well divided) he may be sure that flexure takes place; but he cannot infer the converse, that flexure does not take place, from his obtaining with all the stars the same error in the line of collimation. For if the flexure be the same on both sides of the zenith, a supposition by no means improbable, the observer will then have no indication of flexure by the usual method of determining the error of collimation by stars of different altitudes. Let us suppose that, with an instrument liable to flexure, it is required to measure by both methods the meridional distance of any two stars. The angular distance of the direct images will (as we have already seen) be affected by the difference, or by the sum of two flexures, according as the stars are placed on the same, or on opposite sides of the zenith. In viewing the reflected images, the instrument receiving two new positions, will be subject to two new flexures, by the sum or difference of which (as it may happen) the angular distance of the reflected images will be affected.

The most probable supposition to be made concerning the flexures is, that at equal inclinations with the horizon, above and below it, they will be the same nearly both in direction and degree, and therefore that the two images below the horizon will approach by nearly the same quantity that the direct images receded, or *vice versâ*. With an instrument therefore having such a system of flexures, the double altitude of each star will be correctly ascertained; but stars of different altitudes will

give different determinations of the horizontal point. From observations thus obtained, a near approximation to the true angular distance might be inferred, by taking a mean between the distances of the direct and of the reflected images. The least probable supposition concerning the flexures is, that at equal inclinations above and below the horizon, they will be equal, but in opposite directions; the consequence of which would be, that the direct and reflected images would approach to or recede from one another by the same quantity: the double altitudes of each star would be incorrectly given, but every star would give the same determination of the horizontal point. To suppose however the existence of such a system of flexures, would be to suppose that gravity produced the same change of form in the instrument, as if its direction were inverted; and since the horizontal line is that, at which according to the supposed system a contrary flexure will take place, the flexure at or near the horizon should be zero, where, however, according to the known laws of mechanics it ought to be the greatest. Such a system therefore must be considered as mechanically next to impossible.

If then an instrument give the angular distances both by reflection and by direct vision the same, and the same determination of the horizontal line from stars of whatever altitude, there are then only two hypotheses that can be formed respecting such an instrument; either that the flexures are insensible, or that they are such as are absolutely inconsistent with the laws of mechanics. Hence I conclude that the coincidence of the results by direct vision and by reflection, and the uniform determination of the horizontal point, will be the strongest proof of the non-flexure of the instrument, and of the accuracy of both results.*

In illustration of the whole of the preceding observations, let us examine two catalogues, those of Dr. Brinkley, and Mr. Bessel, which have lately much excited the attention of astronomers. It is obvious, by merely inspecting these catalogues, a comparison of which with the Greenwich catalogue I here subjoin, that one, or both, of the instruments used by these astronomers must be erroneous; and it seems to me, that the source of error is the very flexure, the nature and effects of which we have been considering. For if we attend to the differences between these two catalogues, we shall find that the six stars near the equator differ 5″ from one another, whereas the stars near the zenith do not differ above 2·5″. In which direction flexure will affect the zenith distances, is a matter quite accidental, depending on the unequal elevation or depression of the object-end or eye-end of the telescope, in consequence of the

* I must also notice that the method by reflection possesses, in common with instruments turning in azimuth, the advantage of measuring the double of the required angle.

unequal strength of the materials. If we suppose error to exist in each of the catalogues, this cause must have had an opposite influence in the two cases: if we compare the Greenwich observations with those of Dr. Brinkley, we shall arrive at the same conclusion; namely, that the differences must be caused by flexure in one or both of the instruments; since here also we find that the stars in the neighbourhood of the zenith are affected by only half the difference in polar distance, that is observed in the stars near the equator; and the same conclusions may be drawn from comparing the Greenwich observations with those of Mr. Bessel. The polar distances of all the stars in Mr. Bessel's catalogue exceed the polar distances given in the Greenwich catalogue; while those of all the stars in Dr. Brinkley's catalogue as regularly fall short of my determinations. It is not from the casual circumstance of my results being nearly a mean between the results of those two astronomers, that I intend to claim a superior weight of authority for my own; for were this the only ground for preference, I should regard the question as yet undetermined, and should think it my duty to recommend the providing of new and more powerful instruments for ascertaining the truth. But it appears to me that from the observations by reflection, which I have lately made, and from their agreement with my observations by direct vision, that I am entitled to determine the share of error to which each of these two catalogues is liable; not only from the general superiority of the Greenwich circle, which I consider to have been thus proved, but from this peculiar circumstance, that whereas in the two catalogues of Mr. Bessel and Dr. Brinkley, the errors cannot fail to be the greatest in stars near the horizon; by my method of reflection those stars, which are nearest the horizon, must be determined the most correctly, from their double altitudes being measured on the smallest arc.

In stars near the equator, the catalogue of Mr. Bessel differs from that of Dr. Brinkley five seconds; and from the preceding considerations, I think we may venture to conclude that Mr. Bessel's polar distances are too great by about three seconds, and Dr. Brinkley's too small by about two: and since my catalogue differs from the two former from the zenith to the equator in very nearly the same proportion, there can be no reason to doubt that their errors throughout are divided in nearly the same ratio.

With regard to the catalogue for the present period, which accompanies this paper, I beg to state that I consider it only as a very near approximation to the truth, and requiring at least another year's observations, to render it of equal value with that of 1813, which is the result of two years' observations with six microscopes, and in four positions of the telescope.

I am persuaded that the more this subject is considered, the more distinctly it will appear, that if any doubt can be enter-

tained, founded on any circumstance arising out of the Dublin observations, that doubt must relate, not to the accuracy of former catalogues, but to the present position of the stars; since it is with respect to their *present* position that the two instruments are really at variance. This circumstance is very fortunate, as time may confirm the present, or suggest some more satisfactory method of investigation, if what I have now advanced be not thought sufficient for the purpose.

Article IV.

A Discovery of Chloride of Potassium in the Earth. By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

Sir,

A red ferruginous mass, containing veins of a white crystalline matter, part of a block which was said to have been thrown out of Vesuvius during a late eruption, was brought to me, with a request that I would tell what it was.

This red ferruginous rock was a spongy lava, in the substance of which was here and there lodged a crystal of augite or pyroxene of Haüy, or of hornblende.

The white matter filled most of the larger cavities, and was more or less disseminated through nearly the whole of the mass.

It had a saline appearance; a tabular fracture could be seen in it with a lens, and in some few places regular cubical crystals were discernible.

I supposed it to be chloride of sodium, or muriate of ammonia.

Heated in a matrass, it decrepitated slightly, and melted, but little or nothing sublimed.

This white matter dissolved entirely in water. Laid on silver with sulphate of copper, it produced an intense black stain.

Chloride of barium added to the solution caused only a very slight turbidness, due probably to some sulphate of lime which is present.

Tartaric acid occasioned an abundant formation of crystals of tartar. Chloride of platinum immediately threw down a precipitate, and distinct octahedral crystals of the same nature afterwards appeared.

On decomposition by nitric acid, only prismatic crystals of nitrate of potash could be perceived. On a second crystallization, a few rhombic crystals were discovered; but nitrate of potash sometimes presents this form.

It appears from these experiments, that this white saline matter is pure, or nearly pure, chloride of potassium.

I am inclined to attribute its introduction into the lava to sublimation.

As chloride of potassium is a new species in mineralogy, I shall send the specimen to the British Museum.

Article V.

Astronomical Observations, 1823. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Sept. 2. Immersion of Jupiter's first satellite	15h	24′	58·7″	Mean Time at Bushey.
	15	26	19·6	Mean Time at Greenwich.
Sept. 7. Immersion of Jupiter's second satellite	15	05	21	Mean Time at Bushey.
	15	06	42	Mean Time at Greenwich.
Sept. 19. Immersion of Jupiter's first satellite	15	40	01	Mean Time at Bushey.
	15	41	22	Mean Time at Greenwich.

Article VI.

An Abridged Translation of M. Ramond's Instructions for the Application of the Barometer to the Measurement of Heights, with a Selection from his Tables for facilitating those Operations, reduced (where necessary) to English Measures. By Baden Powell, MA. of Oriel College, Oxford.

(*Continued from* p. 177.)

The configuration of the place where the barometer is situated is far from being a matter of indifference to the accuracy of the measurements. We have just seen what influence it has on the temperature; it appears not to have less on the pressure of the atmosphere. A dry and strongly heated plain gives greater velocity to the ascending currents, which is not done by a verdant hill; upon all sides of which the sun does not shine at the same time. Here the barometer will be proportionally higher; in the other case lower. On an insulated peak all currents have an ascending motion given them from passing along its acclivities: they all acquire a compressing power in a narrow and deep valley where they engulph themselves: and the mercury sustains itself constantly above the point at which it would stand in an open plain at the same absolute elevation. I have measured several hundred times the height of Barèges above Tarbes. The town of Tarbes is situated on an extensive plain. The valley of Barèges is a very narrow gorge, surrounded on all

sides by very high mountains. I have always found the result too little. I have since tried to measure the height of the Pic de Midi above Barèges: I am now at the thirty-second trial, and the measurement is always found too great. These two observations, one of which is, as it were, the complement of the other, have concurred most conclusively to persuade me that there really exists in deep valleys a constant compression of the atmosphere, the effect of which is to augment the height of the mercurial column.

I recommend it as highly desirable to repeat observations with care, and on a large scale, in order to examine more closely the decrease of heat and moisture, and the action of ascending and descending currents. Three or four barometers disposed at different intervals of height, might teach us much, and give an unexpected turn to some inquiries; but the differences of elevation must be great; and above all, the stations must be very favourable. To dispose the instruments in this way on the side of a high mountain would, perhaps, be the first expedient we should be led to think of, but assuredly the last to which I would have recourse. Nothing is certain on long acclivities, where the heat of the ground and the inclination of the currents modify in a thousand ways the pressure of the atmosphere and its temperature. We cannot be too careful in discarding from delicate observations even the most distant suspicion of those local perturbations, of which we cannot exactly estimate the amount. The stations to be preferred are eminences well exposed to the air; summits near others, but to a certain point independent; plains of some extent; but no narrow gorges; no peaks greatly above others; and after my experiments at Barèges, I would not place my barometer in a narrow valley, even if I should be reduced to the necessity of seeking a more convenient station at some distance; for the distance has a much less influence on the accuracy of the measurements than the favourable or unfavourable configuration of the places where the instruments are situated.

Barometrical measurements would inspire less distrust if the observations had been always made with the precautions which the nature of the operation indicates; and there would not be so much dispute on the value of the coefficients and the principles of our formulæ, if the disagreements were not in a great measure produced by the confidence which is too often reposed in observations in themselves very defective. In the present state of the science, it would be much better to endeavour to bring to perfection the very difficult art of observing: to study the circumstances which are favourable, to examine and point out the sources of error; to multiply trials with that patience which the minutest precautions will not tire; with that honesty which will not evade difficulties; with that discernment which directs a depth of study proportioned to the difficulties attach-

ing to this sort of observation; to replace in short whatever observations we possess of a doubtful character, by such as are certain, and the circumstances of which have been judiciously appreciated. It will be time enough to dispute, if there be occasion for dispute, when the propositions in question shall be clear, and the facts free from ambiguity.

Thus far I have spoken of the influence which the configuration of the surface of the earth exercises on the variations of the instruments. The irregular modifications of the atmosphere are another source of errors against which we ought to be on our guard. The theory of barometrical measurements supposes the air in a state of perfect equilibrium; its strata superposed in the order of their density; and the decrease of temperature uniform and regular. It is ordinarily so on fine days and in calm weather; but if the air be agitated and divided between opposing winds, this order is disturbed; strata of different densities are intermixed,* and succeed each other in a different order from that of their respective densities; the thermometric mean no longer expresses the mean temperature of the intercepted column of air; the difference of the heights of the barometer ceases to maintain its relation to the difference of elevation; and no formula can satisfy the exactness of mensuration, in a state of things thus opposed to the fundamental supposition.

When this deviation from regularity is manifest, no one needs to be told that this is not the time to obtain exact measurements; and every one will distrust observations made during a storm, in the midst of tempests, and while violent winds are raging in the atmosphere. But this state of disorder may in some cases be perfectly real without being so apparent; and the intermixture † of winds of different densities is a very usual phenomenon, which, however, frequently escapes the attention, and is the origin of a great number of errors from which the most experienced observer does not easily preserve himself. If we have not been able to avoid them, we must endeavour to beware of their existence, in order that we may not repose in an operation a degree of confidence which it does not deserve.

I have treated elsewhere of the influence of the wind on barometrical measurements; and I invite beginners to profit by my experience, and more advanced observers to correct or extend my first outlines. Whatever judgment they may pass, they will probably agree with me in thinking that there are few subjects of research more interesting, and that we cannot have any just idea of the value of an observation if we neglect, in examining it, considerations of such importance.

I have hitherto always found that northern winds tend to raise the mercury, and southern to depress it.

* "S'intercalent."

† "L'intercalation."

In the former case barometrical measurements tend to err in excess; in the latter, in defect.

If the winds which prevail at the two stations be different, the measurement is too great when the more dense wind occupies the lower stratum, and too small when the upper.

Lastly, the errors augment or diminish, *cæteris paribus*, with the horizontal distance of the two stations, and with the height to be measured.

Among the modifications of the atmosphere there is one of the most hidden description, but nevertheless most regular, which has been investigated with difficulty, but which once known can occasion no errors which we have it not perfectly in our power to prevent. It is long since horary oscillations have been perceived in the barometer. It is long since Deluc observed that the different hours of the day are not equally proper for the measurement of heights.

I have observed that any formula can only be really applicable at the precise hour at which we may have made the experiments necessary for the determination of our coefficient; and that, because the coefficient is always affected by a quantity which represents the mean ratio of the weight of the air and its pressure, a ratio essentially variable, and different at every instant of the day.

The coefficient of the formula of M. de Laplace is adapted to the hour of noon. We must, therefore, make observations for the measurements of heights at the hour of noon only. This precept is important, for the errors which result from the application of a coefficient to hours for which it was not calculated, are among the most considerable that we can make. Yet this consideration will by no means prevent us from prolonging a little the time devoted to the operations. The interval between eleven and one o'clock does not exceed the limits which it is reasonable to prescribe to ourselves; but then if we would be exact, we must operate in such a manner as to effect a compensation between the opposite errors which may arise from this source. Before noon the measurements err in defect; after noon, in excess. My practice is, therefore, to make, besides the observation at noon, one or two others before, and as many after, at intervals respectively equal. This method possesses many peculiar advantages: we have time to examine the progress of the instruments; each observation serves as a point of comparison to judge of the others; and the mean term taken between them is in a manner the observation of noon itself, free from those errors which might be introduced by that accidental state of the atmosphere which should predominate at the real moment of that observation.

Lastly, in order to gain from this combination all the advantages of which it is susceptible, it will be advisable that corres-

ponding observations should be made at the same times, and in the same number. The observers will thus see whether their instruments have proceeded in concert; whether their changes have been correspondent; whether their variations have taken place in the same directions. If they should be of opposite kinds, we shall suspect that the local influences have taken the place of the variations of the atmosphere, and we shall suppress the observations which reciprocally condemn each other.

Such are my methods of proceeding. They have often brought my measurements to a degree of precision which leaves nothing further to wish. I recommend the same care, the same precautions, to those who wish to try the merits of the formula, and especially to those who may wish to correct it.

All this is, I allow, minute and difficult, and this is not, perhaps, the idea we usually form of the nature of barometric measurements. We probably wish that there should be nothing but what is easy in the use of instruments which we employ so commonly; yet what method of measurement is there which has not its uncertainties, its unfavourable times, and even greater difficulties? On the side of the barometer there is always the advantage of simplicity of apparatus, quickness of operation, facility of calculation, the most varied and extensive applications, and a much less dependance on circumstances which put obstacles in the way of using other instruments.

I will now reduce into a brief summary the requisite conditions for the measurement of heights.

1. To employ instruments which correspond; are well constructed; verified with care; and rigorously compared.

2. To choose stations as good as the nature of the places will admit.

3. To allow as little horizontal distance between the two observers as possible; but subordinately to the suitableness of the stations. It may be several leagues without being too great, if the difference of level be considerable, and if there be not between the two stations any eminence which rises above both. The proximity of the stations, on the contrary, will cause more inconvenience than advantage, if the lower barometer is badly situated.

4. To make observations always simultaneous, and exclusively at noon, or between the hours of eleven and one.

5. To choose in general a time when the air is calm rather than when agitated; but not to fear wind when it is gentle and regular. It then renovâtes the local mass of air, and reduces the thermometers to the temperature of the atmosphere.

6. Not to fear a cloudy sky, except when it threatens stormy weather. The suppression of the solar radiation is favourable to the observations, especially if they are made in places freely exposed to the air, and if the instruments have no shelter.

7. To avoid rain, storms, and violent winds; and to be distrustful in uncertain weather, when approaching changes are

indicated by the frequent variations of the barometer and thermometer.

8. To prefer times when the barometer is near its mean height, rather than its extremes.

9. To give continual attention to the variations of the thermometers. The mistakes made in estimating the real temperature of the mercury and the air are the origin of the most considerable and the most common errors.

10. To pay not less constant attention, both to the dispositions of the atmosphere, and to local influences which may affect the accuracy of the measurements. To take exact notice of the direction of the winds, the movements of the clouds, the presence or absence of the sun, and to observe the variations of the instruments in relation to these circumstances.

11. To be doubtful of operations made in very changeable weather, and especially if the air is not uniformly modified at the two stations, as happens when different winds prevail at each; when one enjoys the sun, while the other is clouded, or encompassed with mists; when the decrease of temperature is nothing, or inverted, &c.

If the constitution of the day is such as to be remarkable by any thing excessive, either in the temperature, or in the elevation or depression of the barometer, to repeat the operation in ordinary weather in order to verify the former result; or, in circumstances directly opposite, to correct by compensation contrary errors.

12. If the horizontal distance be very great, to repeat the operation several times. If it be excessive, to rely only on means deduced from a great number of observations always simultaneous. Less than a year will not be sufficient to determine small differences of elevation between places very distant; and if the distance be such that the climates of the respective places should be sensibly different, no barometric mean will determine exactly their relative elevation.

13. To conform to these rules: and in so doing, to use in the observations precision and dexterity; in the examination of all circumstances, to take a just view, and use sound discrimination; and then I can venture to answer that the observer will not be deceived either by the instrument, or the formula.

If circumstances should positively require the sacrifice of any of the prescribed conditions, we shall judge of the merits of the operation by the value of the condition sacrificed.

But it may be asked, shall we content ourselves with mere approximate measurements? Then only, we may reply when we observe as well as circumstances permit. Approximate measurements are not to be disregarded when we only take them as such; and when we have not the means of procuring better. It is still a great instance of utility in the barometer to teach us in an instant, and without difficulty, that which with much apparatus and loss of time, other instruments will not often teach us equally well.

In continuation of this brief summary of what M. Ramond considers the most necessary rules for conducting the observations, I will here, for the sake of those who may be less versed in the use of tables and formula, reduce into rules the method to be observed in performing the calculations.

1. From the common tables of logarithms take the log. of the height of the lower barometer.
2. Apply the correction according to its sign from Table I.
3. From this result subtract the logarithm of the upper barometer.
4. Take the logarithm of this difference supposing it a natural number.
5. Add the logarithm of the constant coefficient.
6. ———————— correction, Table II.
7. ———————————————— Table III.
8. ———————— correction for temperature described in the second section.

The natural number corresponding to the resulting logarithm is the true elevation in feet.

I subjoin an example.

If the shorter formula is used, the seventh step will be dispensed with.

Example.

	Bar. in inch.	Bar. in metres.	Centigrade. Ther. of bar.	Centigrade. Air therm.	Lat.
Lower station....	29·882	·759	15	14	60
Upper station....	24·409	·620	10	6	
			Diff. 5	Sum 20	
				Double 40	

Log. of lower barom. in inches....	1·4754097	Log. of metres	$\bar{1}$·8802418
+ correction, Table I..........	9·9995990		9·9995990
	1·4750087		$\bar{1}$·8798408
— log. upper barom. in inches....	1·3875500	—Log. of metres	$\bar{1}$·7923823
	0·0874587		= 0·0874585 *

Log. of difference of logs.........	$\bar{2}$·9417995
Log. of constant coefficient........	4·7792962
Correction for latitude. Table II.	9·9993835
Correction, Table III. Diff. of logs. ·08. Double sum of the thermometers 40	0·0012323
Corr. for temp. $\frac{1000+40}{1000}=1{\cdot}040$ its log. = ...	0·0170333
	3·8916448 log. of 7791·9 ft. = height required.

* The same result would obviously be obtained, by whatever scale the barometers were divided.

On the Tables.

It may be useful here to subjoin a brief account of the construction of the tables.

In order to reduce the upper barometer to the same temperature as the lower, the formula, transformed into logarithms, is,

$\log. H = \log. h' + \log. \left[1 + \left(\frac{T - T'}{5412}\right)\right].$

Table, No. I. expresses the values of this last term, which becomes $= \log. \left[\frac{5412 + (\tau - \tau')}{5412}\right] = \log. \left(5412 + (\tau - \tau')\right) -$ log. 5412.

In the formula, this correction is applied to the *upper* barometer, and it is obvious that, according as the first log. is greater or less than the second, (that is, according as T is greater or less than T'), the correction will be + or −.

By attending to these circumstances, it may be applied to the *lower* barometer, and will in this case be − or +, according as the difference of the thermometers is + or −.

M. Ramond adopts the method of correcting the lower barometer, and to facilitate this, he has given Table I. a double form, according as the difference of the thermometers is + or −; the one series of numbers being the arithmetical complements of the others, by which means the operation is always addition.

Table No. 2 is constructed from the part of the formula log. $\{1 + N (\log. \cdot 0028371 + \log. \cos. 2 \psi)\}$, N being the number answering to the logarithm included in the parenthesis.

The last factor in the formula when altered according to the suggestion of M. Oltmans, becomes,

$$\text{(in feet), } 1 + \frac{\left(\log. \frac{h}{H} + 0{\cdot}868589\right) . 60158 . 39}{20881129{\cdot}44}.$$

Then according to M. Ramond's improvement, introducing the correction for temperature, and transforming it into logarithms, it becomes, (adopting the former notation),

$$\log. \left[1 + N\left(\log. \left\{\log. \frac{h}{H} + 0{\cdot}868589\right\} + \log. 60158 + \log. \left(1 + \frac{2t + t'}{1000}\right) - \log. 20881129.\right)\right]$$

From this part of the formula, Table No. III. is constructed.

Table No. V. is taken from one given by Laplace in the "Connaisance des Temps," for 1812. It supposes the interior diameter of the tube to be accurately known.

Table No. VI. is described p. 105.

Logarithms of the constant Coefficients.

Log. 60158·39 = 4·7792962
Log. 60345·40 = 4·7806442

The last value is to be used in cases where the less exact method is thought sufficient; and in this case Table III. is dispensed with.

Table I.

For the Reduction of the Barometers to the same Temperature.

Diff. of therm.	Difference Positive, when lower Barometer is the warmer.									
Degrees.	Dec. 0	1	2	3	4	5	6	7	8	9
0°	0·0000000	9920	9840	9759	9679	9599	9519	9438	9358	9278
1	9·9999198	9117	9037	8957	8877	8797	8716	8636	8556	8476
2	8395	8315	8235	8155	8075	7994	7914	7834	7754	7674
3	7593	7513	7433	7353	7273	7192	7112	7032	6952	6872
4	6791	6711	6631	6551	6471	6391	6310	6230	6150	6070
5	5990	5909	5829	5749	5669	5589	5509	5428	5348	5268
6	5188	5108	5028	4948	4867	4787	4707	4627	4547	4467
7	4386	4306	4226	4146	4066	3986	3906	3826	3745	3665
8	3585	3505	3425	3345	3265	3185	3104	3024	2944	2864
9	2784	2704	2624	2544	2463	2383	2303	2223	2143	2063
10	1983	1903	1823	1743	1662	1582	1502	1422	1342	1262
11	1182	1102	1022	0942	0862	0782	0701	0621	0541	0461
12	0381	0301	0221	0141	0061	9981	9901	9821	9741	9661
13	9·9989581	9501	9420	9340	9260	9180	9100	9020	8940	8860
14	8780	8700	8620	8540	8460	8380	8300	8220	8140	8060
15	7980	7900	7820	7740	7660	7580	7500	7420	7340	7260
16	7180	7100	7020	6940	6860	6780	6700	6620	6540	6460
17	6380	6300	6220	6140	6060	5980	5900	5820	5740	5660
18	5580	5500	5420	5340	5260	5180	5100	5020	4940	4860
19	4780	4700	4620	4540	4460	4380	4300	4220	4140	4060
20	3980	3900	3820	3741	3661	3581	3501	3421	3341	3261
21	3181	3101	3021	2941	2861	2781	2701	2621	2542	2462
22	2382	2302	2222	2142	2062	1982	1902	1822	1742	1662
23	1583	1503	1423	1343	1263	1183	1103	1023	0943	0863
24	0784	0704	0624	0544	0464	0384	0304	0224	0144	0065
25	9·9979985	9905	9825	9745	9665	9585	9505	9426	9346	9266
26	9186	9106	9026	8946	8867	8787	8707	8627	8547	8467
27	8387	8308	8228	8148	8068	7988	7908	7829	7749	7669
28	7589	7509	7429	7350	7270	7190	7910	7030	6950	6871
29	6791	6711	6631	6551	6471	6392	6312	6232	6152	6072
30	5993	5913	5833	5753	5673	5594	5514	5434	5354	5274

Table I.

Diff. of therm.	Difference Negative.									
Degrees.	Dec. 0	1	2	3	4	5	6	7	8	9
0	0·0000000	0080	0160	0241	0321	0401	0481	0562	0642	0722
1	0802	0883	0962	1043	1123	1203	1284	1364	1444	1524
2	1605	1685	1765	1845	1925	2006	2086	2166	2246	2326
3	2407	2487	2567	2647	2727	2808	2888	2968	3048	3128
4	3209	3289	3369	3449	3529	3609	3690	3770	3850	3930
5	4010	4091	4171	4251	4331	4411	4491	4572	4652	4732
6	4812	4892	4972	5052	5133	5213	5293	5373	5453	5533
7	5614	5694	5774	5854	5934	6014	6094	6174	6255	6335
8	6415	6495	6575	6655	6735	6815	6896	6976	7056	7136
9	7216	7296	7376	7456	7537	6717	7697	7777	7857	7937
10	8017	8097	8177	8257	8338	8418	8498	8578	8658	8738
11	8818	8898	8978	9058	9138	9218	9299	9379	9459	9539
12	9619	9699	9779	9859	9939	0019	0099	0179	0259	0339
13	0·0010419	0499	0580	0660	0740	0820	0900	0980	1060	1140
14	1220	1300	1380	1460	1540	1620	1700	1780	1860	1940
15	2020	2100	2180	2260	2340	2420	2500	2580	2660	2740
16	2820	2900	2980	3060	3140	3220	3300	3380	3460	3540
17	3620	3700	3780	3860	3940	4020	4100	4180	4260	4340
18	4420	4500	4580	4660	4740	4820	4900	4980	5060	5140
19	5220	5300	5380	5460	5540	5620	5700	5780	5860	5940
20	6020	6100	6180	6259	6339	6419	6499	6579	6659	6739
21	6819	6899	6979	7059	7139	7219	7299	7379	7478	7538
22	7618	7698	7778	7858	7938	8018	8098	8178	8258	8338
23	8417	8497	8577	8657	8737	8817	8897	8977	9057	9137
24	9216	9296	9376	9456	9536	9616	9696	9776	9856	9935
25	0·0020015	0095	0175	0255	0335	0415	0495	0574	0654	0734
26	0814	0894	0974	1054	1133	1213	1293	1373	1453	1533
27	1613	1692	1772	1852	1932	2012	2092	2171	2251	2331
28	2411	2491	2571	2650	2730	2810	2890	2970	3050	3129
29	3209	3289	3369	3449	3529	3608	3688	3768	3848	3928
30	4007	4087	4167	4247	4327	4406	4486	4566	4646	4726

Table II.—*Latitudinal Diminution of Gravity.*

Latitude.	Logarithms.	Latitude.	Logarithms.	Latitude.	Logarithms.
0	0·0012304	26	0·0007579	51	9·9997438
1	12296	27	7236	52	7018
2	12274	28	6884	53	6603
3	12237	29	6524	54	6191
4	12184	30	6156	55	5784
5	12117	31	5781	56	5383
6	12035	32	5398	57	4986
7	11939	33	5009	58	4596
8	11828	34	4613	59	4212
9	11702	35	4212	60	3835
10	11563	36	3806	61	3466
11	11409	37	3395	62	3104
12	11242	38	2980	63	2752
13	11060	39	2561	64	2408
14	10866	40	2139	65	2073
15	10658	41	1714	66	1748
16	10437	42	1288	67	1432
17	10203	43	0859	68	1128
18	0·0009957	44	0430	69	0834
19	9699	45	0000	70	0551
20	9429	46	9·9999570	71	0280
21	9147	47	9140	72	0021
22	8854	48	8712	73	9·9989774
23	8521	49	8285	74	9539
24	8237	50	7860	75	9316
25	7913				

Table V.—*Capillary Depression of Mercury.*

Diam. of Tube.	Depression.
Inches.	Inches.
0·08	0·179
0·12	0·114
0·16	0·080
0·20	0·058
0·24	0·045
0·28	0·034
0·32	0 026
0·36	0·020
0·40	0·016
0·44	0·013
0·48	0·009
0·52	0·008
0·56	0·006
0·60	0·004
0·64	0·003
0·68	0·003
0·72	0·002
0·76	0·002
0·80	0·001

Table III.—*Vertical Diminution of Gravity.*

Difference of the Logarithms.	Double Sum of the Thermometers. −10°	0	+10°	20°	30°	40°
0·005	0·0010805	10914	11023	11132	12241	11350
0·01	0·0010867	10977	11086	11196	11305	11415
0·02	10990	11102	11212	11323	11433	11545
0·03	11114	11226	11338	11450	11562	11674
0·04	11237	11351	11464	11578	11690	11804
0·05	11361	11476	11590	11705	11819	11934
0·06	11484	11601	11716	11832	11947	12064
0·07	11608	11725	11842	11959	12076	12193
0·08	11731	11850	11968	12086	12204	12323
0·09	11865	11975	12094	12214	12333	12453
0·10	11978	12099	12220	12341	12461	12582
0·11	12102	12224	12346	12468	12590	12712
0·12	12225	12349	12472	12595	12718	12842
0·13	12349	12473	12598	12722	12847	12971
0·14	12472	12598	12724	12850	12975	13101
0·15	12596	12723	12850	12977	13104	13231
0·16	12719	12848	12976	13104	13232	13361
0·17	12842	12972	13101	13231	13360	13490
0·18	12966	13097	13227	13358	13489	13620
0·19	13089	13222	13353	13486	13617	13750
0·20	13213	13346	13479	13613	13746	13879
0·21	13336	13471	13605	13740	13874	14009
0·22	13460	13596	13731	13867	14003	14139
0·23	13583	13720	13857	13994	14131	14268
0·24	13707	13845	13983	14122	14260	14398
0·25	13830	13970	14109	14249	14388	14528
0·26	13954	14095	14235	14376	14517	14658
0·27	14077	14219	14361	14503	14645	14787
0·28	14201	14344	14487	14630	14774	14917
0·29	14324	14469	14613	14758	14902	15047
0·30	14448	14593	14739	14885	15031	15176
Mean difference of the terms.	123·5	124·7	126·0	127·2	128·5	129·7

TABLE III.—*Continued.*

Difference of the Logarithms.	Double Sum of the Thermometers. 50°	60°	70°	80°	90°	100°	Mean difference of the Terms.
0·005	11458	11567	11676	11785	11894	12003	108·9
0·01	11524	11634	11743	11853	11962	12072	109·5
0·02	11655	11766	11876	11988	12098	12209	110·8
0·03	11786	11898	12010	12122	12234	12346	112·0
0·04	11917	12031	12143	12257	12370	12484	113·4
0·05	12048	12163	12277	12392	12506	12621	114·5
0·06	12179	12295	12401	12526	12642	12758	115·8
0·07	12310	12427	12544	12661	12778	12895	117·0
0·08	12441	12559	12677	12796	12914	13032	118·3
0·09	12571	12691	12810	12930	13049	13169	119·5
0·10	12702	12824	12945	13065	13185	13306	120·7
0·11	12833	12956	13077	13200	13321	13444	122·0
0·12	12964	13088	13211	13334	13457	13581	123·3
0·13	13095	13220	13344	13469	13593	13718	124·5
0·14	13226	13352	13478	13604	13729	13855	125·7
0·15	13357	13484	13611	13738	13865	13992	126·9
0·16	13488	13617	13744	13873	14001	14130	128·3
0·17	13619	13749	13878	14008	14137	14267	129·5
0·18	13750	13881	14011	14142	14273	14404	130·7
0·19	13881	14013	14145	14277	14409	14541	132·0
0·20	14012	14145	14278	14412	14545	14678	133·2
0·21	14143	14277	14412	14546	14681	14816	134·5
0·22	14274	14410	14545	14681	14817	14953	135·7
0·23	14405	14542	14679	14816	14953	15090	137·0
0·24	14535	14674	14812	14950	15088	15227	138·2
0·25	14666	14806	14945	15085	15224	15364	139·5
0·26	14797	14938	15079	15220	15360	15501	140·6
0·27	14928	15070	15212	15354	15496	15638	141·9
0·28	15059	15203	15346	15489	15632	15776	143·2
0·29	15196	15335	15479	15624	15768	15912	144·4
0·30	15321	15467	15612	15758	15904	16050	145·6
Mean difference of the terms.	130·9	132·2	133·4	134·7	135·9	137·2	

TABLE VI.—*Correction for Elevation of Lower Barometer.*

Lower Barom.	Number.	Diff.
29·5	130	
27·5	955	825
25·5	1824	869
23·6	2735	911
21·6	3733	998
19·6	4818	1085
17·7	6033	1215
15·7	7420	1387

Table IV.—*Thermometrical Variation of the Barometer.*

Barom.	Differences of Temperature.									
	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°
Inches.										
31·0	005	·011	·017	·022	·028	·034	·040	·045	·051	·057
30·5			·017	·022	·027	·033	·039	·044	·050	·056
30·0			·017	·022	·027	·033	·039	·044	·049	·055
29·5			·016	·021	·026	·032	·038	·043	·048	·054
29·0			·016	·021	·026	·032	·037	·042	·047	·053
28·5			·016	·021	·025	·031	·037	·042	·047	·052
28·0		·010	·015	·020	·025	·031	·036	·041	·046	·051
27·5			·015	·020	·024	·030	·035	·040	·045	·050
27·0	004		·015	·020	·024	·030	·035	·040	·045	·049
26·5			·015	·019	·023	·029	·034	·039	·044	·048
26·0			·014	·019	·023	·029	·034	·038	·044	·047
25·5		·009	·014	·019	·023	·028	·033	·038	·043	·046
25·0			·014	·018	·022	·028	·032	·037	·042	·045
24·5			·013	·018	·022	·027	·032	·036	·041	·044
24·0			·013	·017	·021	·027	·031	·036	·040	·044
23·5			·013	·017	·021	·026	·030	·035	·040	·043
23·0		·008	·012	·017	·020	·026	·029	·034	·039	·042
22·5	·003		·012	·016	·020	·025	·029	·034	·038	·041
22·0			·012	·016	·019	·025	·028	·033	·037	·041
21·5			·011	·016	·019	·024	·028	·033	·036	·040
21·0			·011	·015	·019	·023	·027	·032	·035	·039
20·5		·007	·011	·015	·018	·022	·026	·031	·034	·038
20·0			·011	·015	·018	·022	·026	·030	·033	·037
19·5			·010	·014	·017	·021	·025	·029	·032	·036
19·0			·010	·014	·017	·021	·024	·028	·031	·035
18·5		·006	·010	·013	·016	·020	·024	·027	·030	·034
18·0			·010	·013	·016	·020	·023	·026	·029	·033
17·5			·009	·012	·015	·019	·022	·025	·028	·032
17·0			·009	·012	·015	·019	·021	·024	·028	·031
16·5		·005	·009	·012	·014	·018	·021	·023	·027	·030
16·0			·008	·011	·014	·018	·020	·022	·026	·029
15·5	2		·008	·011	·013	·017	·019	·022	·025	·028
15·0			·008	·011	·013	·017	·019	·021	·025	·027
14·5			·007	·010	·012	·016	·018	·021	·024	·026
14·0			·007	·010	·012	·016	·018	·020	·023	·025
13·5		·004	·007	·010	·012	·015	·017	·019	·022	·025
13·0			·006	·009	·011	·015	·017	·019	·021	·024
12·5			·006	·009	·011	·014	·016	·018	·020	·023
12·0			·006	·009	·010	·013	·016	·017	·019	·022

One great excellence of M. Ramond's tables consists in their being (with a very slight exception) applicable to any system of measures. They are, however, adapted to the centigrade thermometric scale. In this respect I have not altered them, being convinced that the simplicity and convenience of that scale must sufficiently recommend it in all scientific applications; and having little doubt that the example of its adoption by philosophers will ultimately be followed by the world at large. The adoption of a system of *measures* founded on a philosophical basis, will probably always be hindered by the close and widely ramified connexion which the old system maintains with all parts of the common business of life, and with the lowest mechanical arts. It will probably be long before our carpenters, bricklayers, and blacksmiths, will learn to compute by decimetres and centimetres; but with respect to the introduction of the centigrade thermometer, the same objections by no means apply. The thermometer is an instrument which has no application in these common arts. The most ordinary use of it implies a *certain degree* of education and scientific information; and to those not habituated to scientific studies, but who are yet desirous of understanding the principle of their instrument, the centigrade scale is surely far the best for facility of explanation. In barometric observations, however, its application is now becoming so general that I conceive no explanation is necessary for continuing the adoption of this scale in these tables. Most mountain barometers, as at present constructed, are furnished with a thermometer graduated both ways. For the convenience, however, of those who continue to use the Fahrenheit scale, I here insert a table of the simplest and most compendious form, by which either scale may be reduced to the other with the greatest ease and sufficient accuracy.

Reduction of Cent. to Fahr.		Reduction of Fahr. to Cent.	
Cent. Degrees or Dec.	Fahr. Degrees or Dec.	Fahr. Degrees or Dec.	Cent. Degrees or Dec.
00	00	0	000
05	09	1	055
10	18	2	111
15	27	3	166
20	36	4	222
25	45	5	277
30	54	6	333
35	63	7	388
40	72	8	444
45	81	9	500

The mode of using it will be obvious from the subjoined examples:

Example 1.—27° Cent.

$$\left.\begin{array}{r} =25 \\ +\ \ 2 \end{array}\right\}\ldots\ldots\left\{\begin{array}{r} 45{\cdot}0\text{ Fahr.} \\ 3{\cdot}6 \end{array}\right.$$

$$\begin{array}{r} 48{\cdot}6 \\ +\ 32{\cdot}0 \\ \hline 80{\cdot}6 \end{array}$$

66° Fahr.

$$\begin{array}{r} -\ 32 \\ \hline 34 \end{array} \quad \left.\begin{array}{r} 30 \\ 4 \end{array}\right\}\ldots\left\{\begin{array}{r} 16{\cdot}6\text{ Cent.} \\ 2{\cdot}2 \\ \hline 18{\cdot}8 \end{array}\right.$$

Example 2.—33·25 Cent.

$$\left.\begin{array}{r} =30{\cdot}00 \\ +\ \ 3{\cdot}00 \\ +\ \ 0{\cdot}25 \end{array}\right\}\ldots\ldots\left\{\begin{array}{r} 54{\cdot}0\text{ Fahr.} \\ 5{\cdot}4 \\ 0{\cdot}45 \end{array}\right.$$

$$\begin{array}{r} 59{\cdot}85 \\ +\ 32{\cdot}0 \\ \hline 91{\cdot}85 \end{array}$$

48·75 Fahr.

$$\begin{array}{r} -\ 32 \\ \hline 16 \end{array}$$

$$\left.\begin{array}{r} 10{\cdot}0 \\ 6{\cdot}0 \\ 0{\cdot}7 \\ 0{\cdot}05 \end{array}\right\}\ldots\left\{\begin{array}{l} 5{\cdot}5\text{ Cent.} \\ 3{\cdot}3 \\ 0{\cdot}388 \\ 0{\cdot}027 \end{array}\right.$$

$$\begin{array}{r} \hline 9{\cdot}215 \end{array}$$

Having thus brought to a conclusion an attempt to condense into a brief abstract the most useful parts of M. Ramond's Instructions, I propose, in a future communication, to subjoin an outline of the demonstration of the formula; together with a few remarks on one or two other points connected with the subject. B. P.

(To be continued.)

Article VII.

Notice upon the Volcanic Island of Milo. By Sir Francis S. Darwin, MD. &c. (With Plate XXII.)

(To the Editor of the *Annals of Philosophy*.)

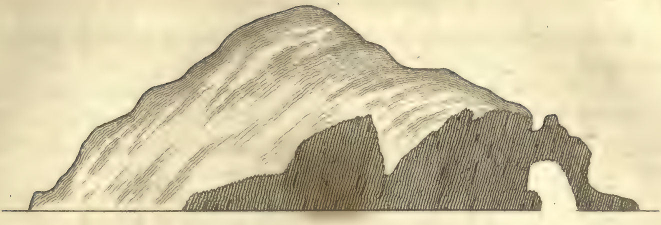
Sir, *Buxton, Aug.* 20, 1823.

Observing your analysis of the water from what you call the boiling spring of Milo, in the July number, 1819, of the *Annals of Philosophy*,* I presume that the following description of that island, taken from my manuscript journal, may be interesting; I beg to offer it, and shall be happy if it affords any information to your readers.

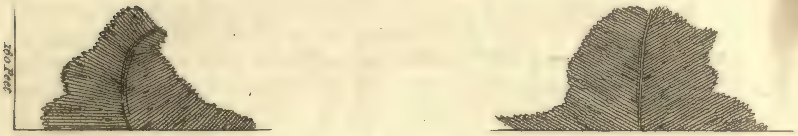
I remain, Sir, your obedient humble servant,

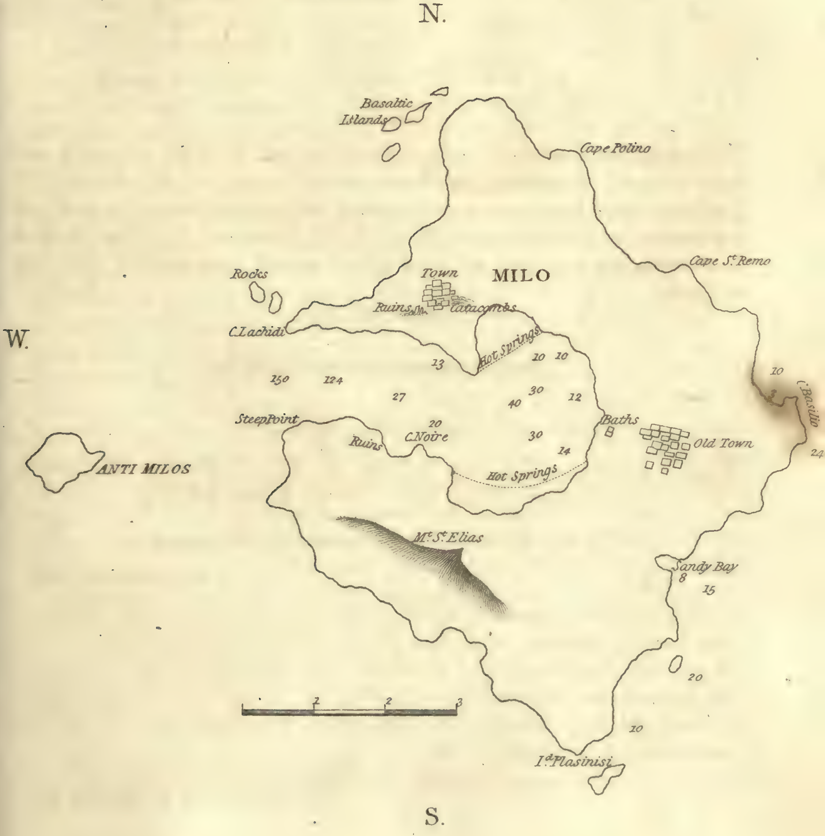
Francis S. Darwin.

* O. S. xiv. 27: see also p. 68 of the present volume.



SMALL BASALTIC ISLANDS NEAR MILO.





Engraved for the Annals of Philosophy: for Baldwin, Cradock & Joy, Octr. 1 1823.

In the month of June, 1810, we landed at Milo, and proceeded to the Old Town, which is quite ruinous, although a few inhabitants still occupy some of the houses. The ancient walls bear the marks of great waste and decay. Four miles from this is the New Town, situated upon one of the most elevated parts of the island. The incursions of the Algerines, and the plague, induced these few hundred people to neglect their former low situation (the old city) for this elevated one. On the foot, and at the side next the sea, of the hill upon which the present town stands, are many very interesting remains of a most remote period. We could distinctly trace the extent of a large amphitheatre, and many beautiful marble columns are seen amongst the ruins. Walls of immense thickness;—and the cement by which these stones are held together, appears to brave the waste of time better than the hard stone itself. The highest point of Milo, or Mount St. Elias, is about 800 feet above the sea, and it is of a conical shape; this summit was formerly a place of observation for pilots, but now for pirates, who infest the Archipelago. On the north side of the island, and half a mile from it, are some curious basaltic rocks, which do not appear to contain zeolite; but there are no columns on Milo itself. Upon ascending from the harbour to the town, we pass over hills and rocks of lava, in which opal is found, with pumice stone, and sulphur, and beds of limestone which have been burnt, and still retain many perfect shells, which soon absorb moisture, and fall to pieces in the hand. In one part of a rock of red sandstone, at about the middle of the entrance into the port, are some singular Catacombs in the perpendicular rock, some of them capable of containing four, six, or eight bodies, and they are seen in the side of the cliff ten or twenty feet below the level of the water. This is a strong proof that the harbour was the crater of a volcano, as here there is no tide, and these tombs must have been formed before the grand eruption which gave access to the sea. The situation of these sepulchres is marked upon the chart. It occurred to me on seeing in the map (which was partly copied from an Admiralty book), that 40 fathoms was the greatest depth in this large basin, that there might be a part infinitely deeper which had been the real furnace of the volcano. I was at great pains in sounding, but could no where find it to be deeper, except at the entrance. On the west and south sides of the harbour are innumerable hot sulphureous springs, some of them being 125° of Fahr. but most of them rise out of the sand, in the sea a few yards from the shore; they are so numerous that every wave, although it blows fresh, is very warm to the hand. Along with the water, a great quantity of sulphuretted hydrogen gas is emitted. The ruins of ancient baths still exist here, and near them a part of an inscription, with the name Diagoras. Now if the eruption had taken place since the time of that philosopher

(about 400 years before Christ), we should probably have had some records of it; therefore it is fair to presume that the Catacombs are of more ancient date. I obtained an ancient Greek vase taken from one of these sepulchres, which has all the characters of the very earliest period of the arts.

The island is still subject to frequent earthquakes; and probably it was an exertion of this volcano, or of that at Santorini, which destroyed one of the principal towns of Candia or Crete, with its inhabitants, in the year 1809.

About four miles to the north-east of Milo is Polino (or Burnt Island), which consists entirely of one immense cinder, with a central hill composed of a smooth-fractured, compact, baked clay, of a dusky white colour, appearing like a heap of pottery, and the highest point being about 500 feet above the sea. To ascend this hill, it is necessary to pass along a ridge, of which there are eight, that support the central mass. The intervals of these ridges form ravines of pumice stone. The island is uninhabited, and entirely without fresh water, which is not the case with Milo, where there are springs of good water. Upon ascending the hill of Polino, I observed some modern excavations, like mines, but there is not any apparent metallic substance that could have been followed, and they extend about 20 yards into the hill, showing the invariable volcanic formation of the island. There are here no remains of ancient ruins, and there is scarcely any vegetation.

Article VIII.

An Examination of the Blood. By J. L. Prevost, MD. and J. A. Dumas.*

The authors commence their memoir with observing, that the previous microscopic examination of the blood had proved that this fluid during life is merely serum, holding small regular insoluble corpuscules in suspension. These corpuscules are always composed of a central colourless spheroid, inclosed in a red coloured membrane, from which it readily separates after death. This white central spheroid is transparent and spherical in those animals which have circular particles, and oval when the particles are elliptical In the first case its diameter is constant; in the second, various. The colouring matter is readily divisible, but insoluble in water, and always separates from it by standing.

The three substances which are examined in the chemical

* Abstracted from the Annales de Chimie et de Physique, tome xxiii. p. 50.

investigation of the blood, are the albumen, the serum, the white globule, and the colouring matter. White of egg is albumen nearly pure, but the serum of ox or sheep's blood is purer, as the white of egg always contains light membranous flocculi, which are not albumen.

The coagulation of albumen by heat being so characteristic a property, and the cause of it difficult to discover, experiments were made to determine the circumstances which accompany it. By heating white of egg in tubes placed in water over a lamp, the following results were obtained:

At 140° Fahr. the white of egg remained thin and clear.

145° an opalescent tint appeared at the lower part of the tube.

149° the opalescent portion became solid, while the upper portion remained fluid.

158° the opalescent appearance occurred in the upper part of the tube.

165° the solidification was complete.

The authors conclude that 158° is about the coagulating point; coagulated albumen, when examined by the microscope, presents the same white globules which have been already mentioned. None of the circumstances which accompany the coagulation of albumen lead to a discovery of its cause. The authors then refer merely to the opinions of Fourcroy and Scheele as being erroneous. M. Thenard's opinion, that it is derived merely from the cohesion of the molecules of the albumen, they consider it difficult to substantiate by experiment; and they also think it possible, but not easily demonstrable, that the caustic soda necessary to the solution of the albumen may become carbonate by the decomposition of a small portion of animal matter, and so become incapable of retaining the albumen in solution. The authors then remark, that the action of voltaic electricity elucidates the state of combination which exists between the albumen and the soda; many other well-known experiments, they observe, show that this substance is also capable of combining with metallic oxides. When a metallic salt is precipitated by albumen, a portion of the acid is retained by the oxide, and all the oxide is not in combination with the animal matter, for the soda of the albumen decomposes a part of the salt, independently of it. When the decomposition of albumen is effected by the pile, with a copper wire, a compound is obtained, which consists of water, albumen, and oxide of copper: when moist, it is slightly green, and when dried, of a turquoise colour. If an iron wire be employed, then a compound of albumen and oxide of iron is obtained. The coagulation of albumen by alcohol is owing to the affinity of this fluid for soda; and it is stated to be the best mode of procuring albumen in a state of purity. When examined by reagents, it does

not appear to differ at all from fibrin. The action of acids upon albumen leads to the same conclusion, although there are two different operations to be distinguished; first, the saturation of the soda; secondly, the action of the acid upon the albumen. The first explains the precipitation of albumen by the greater number of acids, the action of the acids depending upon their nature; thus acetic and phosphoric acids redissolve, or at least reduce, even fibrin itself to a gelatinous state, and consequently they do not precipitate it from its alkaline solutions.

MM. Prevost and Dumas observe, that the history of the colouring matter of the blood would have been long since settled, if it had not been for an error caused by a very simple circumstance: the colouring matter of the blood, owing to its extreme divisibility when put into water, and to its passing through filters, has been supposed to be soluble in water. By the aid of a microscope, however, the particles are perceptible, and by standing, they separate in the state of a dense red substance. On this account the authors conceive that the action of reagents upon the colouring matter of the blood has never been satisfactory.

The colouring matter of the blood appears to be formed of an animal substance in combination with peroxide of iron. Experiments hitherto made would lead to the conclusion that it is albumen; but as chemists have always operated upon a mixture of red matter, white globules, and the albumen of the serum, the question is undecided, and the authors expressly state their belief, that the processes proposed by MM. Berzelius, Brande, and Vauquelin, to isolate the colouring matter, are all fallacious.

MM. Prevost and Dumas observe, that it is much more easy than it has been supposed, to determine the proportions of the different animal matters which the blood contains, and the following are the results of their experiments:—

Mammiferæ.

Green monkey (*Callitriche*). Blood drawn from the basilica.

	Serum.		Blood.
Water	908	Water	7760
Albumen and salts	92	Particles	1461
		Albumen and salts	779
	1000		10000

Man in a healthy state: venous blood: mean of many analyses.

	Serum.		Blood.
Water	900	Water	7839
Albumen and salts	100	Particles	1292
		Albumen and salts	869
	1000		10000

Man in a healthy state: blood from the vena portæ after execution.

	Serum.		Blood.
Water	905	Water	8014
Albumen and salts	95	Particles	1142
	1000	Albumen and salts	844
			10000

Guinea pig: blood from the jugular.

	Serum.		Blood.
Water	900	Water	7848
Albumen and salts	100	Particles	1280
	1000	Albumen and salts	872
			10000

Dog: blood from the jugular.

	Serum.		Blood.
Water	926	Water	8107
Albumen and salts	74	Particles	1238
	1000	Albumen and salts	655
			10000

Cat.

	Serum.		Blood.
Water	904	Water	7953
Albumen and salts	96	Particles	1204
	1000	Albumen and salts	843
			10000

Goat: blood taken from one of the saphenæ. The blood of this animal is light coloured, and the venous blood nearly as red as the arterial.

	Serum.		Blood.
Water	907	Water	8146
Albumen and salts	93	Particles	1020
	1000	Albumen and salts	834
			10000

Calf: mixture of arterial and venous blood obtained at a slaughter-house.

	Serum.		Blood.
Water	901	Water	8260
Albumen and salts	99	Particles	912
	1000	Albumen and salts	828
			10000

Hare: the blood taken from one of the jugulars.

Serum.		Blood.	
Water	891	Water	8379
Albumen and salts	109	Particles	938
		Albumen and salts	683
	1000		10000

Horse: venous blood.

Serum.		Blood.	
Water	901	Water	8183
Albumen and salts	99	Particles	920
		Albumen and salts	897
	1000		10000

Birds.

Pigeon: blood from the jugular.

Serum.		Blood.	
Water	945	Water	7974
Albumen and salts	55	Particles	1557
		Albumen and salts	469
	1000		10000

Duck: blood from the jugular.

Serum.		Blood.	
Water	901	Water	7652
Albumen and salts	99	Particles	1501
		Albumen and salts	847
	1000		10000

Hen: blood from the jugular.

Serum.		Blood.	
Water	925	Water	7799
Albumen and salts	75	Particles	1571
		Albumen and salts	630
	1000		10000

Raven: a very young bird.

Serum.		Blood.	
Water	934	Water	7970
Albumen and salts	66	Particles	1466
		Albumen and salts	564
	1000		10000

Heron: this bird had been wounded, and had refused food for some days. Only one analysis was made.

	Serum.		Blood.
Water	932	Water	8082
Albumen and salts	68	Particles	1326
		Albumen and salts	592
	1000		10000

Cold-blooded Animals.

Trout.

	Serum.		Blood.
Water	923	Water	8637
Albumen and salts	77	Particles	638
		Albumen and salts	725
	1000		10000

Burbot (*Gadus Lota*).

	Serum.		Blood.
Water	931	Water	8862
Albumen and salts	69	Particles	481
		Albumen and salts	657
	1000		10000

Frog: mixed blood obtained towards the end of winter.

	Serum.		Blood.
Water	950	Water	8846
Albumen and salts	50	Particles	690
		Albumen and salts	464
	1000		10000

Land Tortoise: the animal was bled to death from the jugular towards the end of winter. The blood did not differ in appearance from that of a bird, the clot being bulky. The animal had neither eaten nor drank for five months. Its temperature was exactly that of the air; it breathed only three times in a minute.

	Serum.		Blood.
Water	904	Water	7688
Albumen and salts	96	Particles	1506
		Albumen and salts	806
	1000		10000

Common Eel. Mr. Hewson has stated the globules of the eel to be circular, but they were found to be elliptical. The blood was obtained from the aorta.

	Serum.
Water	900
Albumen and salts	100
	1000

	Blood.
Water	8460
Particles	600
Albumen and salts	940
	10000

MM. Prevost and Dumas observe, that the inspection of these results will prove that it is impossible to draw any general conclusions from them respecting the composition of the serum: this fluid varies in the same animal, and still more in different animals, without the possibility of connecting this character with the physiological condition of the individual. With the particles, the case is different, and, in the greater number of cases, their quantity bears a certain relation to that of the heat developed by vital action, as will appear by the annexed table: it shows the number of particles in 10000 parts of the blood, the usual temperature of the rectum, the number of beats of the heart, and the inspirations in a minute. To complete our knowledge on this subject, we want the relative weights of the animal and the blood in circulation. With this difficult subject the authors are now engaged.

Animal.	Weight of particles in 10000 of blood.	Mean temperature.	Pulsations in a minute.	Respirations in a minute.
Pigeon	1557	107·6 Fahr.	136	34
Hen	1571	106·7	140	30
Duck	1501	108·5	110	21
Raven	1466	—	—	—
Heron	1326	105·8	200	22
Ape	1461	95·9	90	30
Man	1292	102·2	72	18
Guinea Pig	1280	100·2	140	36
Dog	1238	99·3	90	28
Cat	1204	101·3	100	24
Goat	1020	102·6	84	24
Calf	912	—	—	—
Hare	938	100·2	120	36
Horse	920	96·2	56	16
Sheep	900	100·2	—	—
Trout	638	—	—	—
Burbot	481	That of the place.	—	36
Frog	690	48·2 in water of 45·5	—	20
Tortoise	1506	That of the air.	—	3
Eel	600	—	—	—

In attempting a comparative examination of arterial and venous blood, unexpected difficulties and irregular results occurred. It was at length discovered that when a small animal is bled to a considerable extent, the veins rapidly absorb, at the expense of the rest of the system, a proportional, or perhaps an equal quantity of fluid to that in circulation: from which it

follows that the quantity of particles appears to diminish in a given quantity of blood. This was proved by the following experiments:—A cat which had been previously bled for another purpose, had venous blood again taken from it; it contained 862 parts of particles in 10000 of the blood; when bled the next day, the particles amounted to 856 parts. A strong healthy cat was much bled from the carotid; the blood contained 1184 of particles in 10000 parts; two minutes afterwards, blood taken from the external jugular gave 1163 of particles; it was then suffered to bleed five minutes, and blood then taken from the jugular gave only 935 of particles. These two experiments, the authors observe, leave no doubt as to the rapidity of the absorption, and they indicate at the same time the plan to be adopted for avoiding the error which it occasions. It appeared probable that by slightly bleeding a strong animal, no sensible effect would be produced; a sheep being bled for this purpose, from the carotid, 10000 parts gave 935 of particles, and venous blood from the jugular gave 861; the blood of the dog and cat present similar differences. Ten thousand parts of arterial blood usually contain 100 parts more globules than venous blood. Sometimes the serums are similar, sometimes slight uncertain differences are perceptible. Care was taken in the experiments related to obtain the venous before the arterial blood, in order that the venous absorption, if it occurred, should not be in favour of the circumstances related.

In recapitulating the results of their labours, MM. Prevost and Dumas remark,

First, that arterial blood contains more particles than venous blood;

Secondly, that the blood of birds is the most abundant in particles;

Thirdly, that the mammiferæ succeed birds in this respect, and that it would appear that the blood of the carnivorous tribes contains more particles than that of the herbivoræ;

And, fourthly, that cold-blooded animals possess the smallest number of particles.

Finally, they observe, we acquire a direct proof of absorption by the veins after bleeding. We may even make use of this principle for the purpose of explaining the anomaly in the heron. It had lost much blood; it had not taken any food for some days; and it seems fair to conclude from these two circumstances, that the volume of particles having diminished, on the one hand, and not having been replaced, on the other, necessarily remained below the mean.

The apparent anomaly in the tortoise may be explained with equal facility. The life of this animal is almost suspended during winter, so that many fewer particles of its blood are destroyed. It must, however, lose water, partly in respiration; partly by transpiration; and partly by urine, the excretion of which in abundance regularly continues.

Article IX.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 121.)

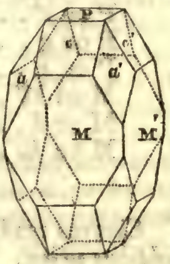
In order to apply successfully the tables of modifications referred to in my last communication, or indeed to compare crystals with any of the engraved figures by which they are represented, it is necessary to acquire a habit of what may be termed *reading* crystals. This is not difficult of attainment when they are regularly formed, but when they are distorted in their shape, and some of the planes which are represented in the drawings as equal and symmetrically placed, are disproportionately enlarged at the expense of others, it requires a little more consideration to trace the character of the true form in the imperfect crystal. It is convenient to attach the crystal we are examining to the end of a bit of wax taper two or three inches long, by means of which we may hold it in any position. Our first object should be to discover some symmetrical lateral or terminal planes, and when we have discovered these, the crystal should be placed on the wax so as to enable its being conveniently held with its lateral planes vertical.

The *cube, tetrahedron,* and all the *octahedrons,* may be easily recognised. The *right square prism* may be distinguished from the *cube* by not having its lateral and its terminal edges similarly modified. In the *right rectangular prism,* the lateral planes incline to each other at an angle of 90°, but in the *right oblique-angled prism* those planes incline alternately at a greater and less angle than 90°; the terminal plane in both is perpendicular to the lateral planes, and the planes which replace the solid angles incline unequally on the three adjacent primary planes. The *right rhombic prism* is distinguishable from the *oblique* by the inclination of the terminal or the lateral planes being 90° in the *right* prism, and being greater and less alternately in the oblique. The planes marked *c* in the *right* prism incline equally on the two adjacent lateral planes, while those marked *e* in the oblique incline unequally on the adjacent planes. The *rhomboid* may be distinguished from the *oblique rhombic prism,* to which it bears a great analogy in its general form, by the symmetry of its modifying planes when held with its axis vertical; and by the equal inclination on the three adjacent lateral planes, of a plane replacing its terminal solid angle; whereas an apparently corresponding plane on the *oblique rhombic prism* will measure unequally on the adjacent lateral planes. But it will be well to procure regularly formed crystals of some of the substances described, and by holding these in the positions in which they are represented in the drawings, the relations of the several

figures to each other will be readily perceived, and the less regularly formed ones will, after a little practice in examining them, be more easily understood.

Chloride of Mercury.—Calomel.

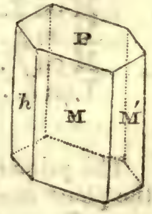
I have received from Mr. Cooper some good crystals of this substance, which have afforded the measurements given below. I have not found any distinct cleavage among these, but there are indications of cleavages parallel to all the planes of *a square prism,* which may be regarded as the primary form. The primary form of the mineral, which has been called *muriate of mercury,* is also *a square prism,* and the secondary planes which replace the terminal edges and angles of a crystal I have measured, incline at the same angles on the lateral planes, as these do.



P on M, or M′	90°	00′
P on *a*	112	5
P on *c*	119	50
M on M′	90	00
M on *c*	150	10
a on edge G.	157	55

Bichloride of Mercury.—Corrosive Sublimate.

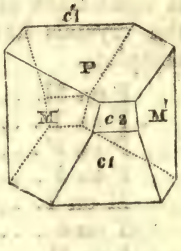
I am obliged to Mr. R. Howard for the crystals from which this form has been determined. The cleavages are parallel to the lateral and to the terminal planes of *a right rhombic prism* of 93° 44′. I have not observed any modifications of the terminal planes from which the dimensions of the primary form may be inferred.



P on M, or M′	90°	00′
M on M′	93	44
M on *h*	133	8

*Phosphate of Ammonia.**

The primary form is *an oblique rhombic prism,* and there are indistinct cleavages parallel to the planes M and M′. The crystals are usually lengthened in the direction of the horizontal diagonal of the figure.



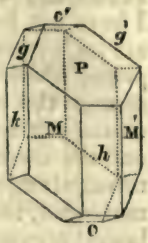
P on M, or M′	105°	50′
P on c_2	92	42
P on c_1	109	32
M on M′	84	15

* For the crystals of this and the five following substances, I am obliged to Mr. Cooper, who informs me that he proposes to make collections of crystals of the artificial salts for sale; and he contemplates that he shall be enabled to supply them at a very moderate price.

Phosphate of Soda.

This salt effloresces so readily, that if it be attempted to be measured in a warm and dry day, the planes will become obscure before they can all be adjusted on the goniometer.*

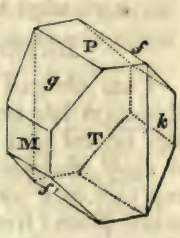
The primary form is *an oblique rhombic prism,* with indistinct cleavages parallel to the planes M and M′. The crystals are frequently deposited singly and very symmetrically formed.



P on M, or M′.	106°	44′
P on c′	129	12
P on g	112	27
P on h.	121	14
M on M′	67	30
M on h	123	45
M on k.	146	15

Succinate of Ammonia.

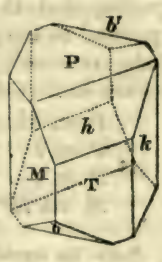
Cleaves readily parallel to the planes P, M, and T, of *a doubly oblique prism.* The attachment of the crystals is commonly by one of the summits of the figure.



P on M.	91°	53′
P on T	93	25
P on k	91	45
P on g	151	57
P on f	151	7
M on T.	100	15
M on g	119	53
M on f′.	117	00
T on k..	135	46

Succinate of Soda.

The primary form is *a doubly oblique prism,* of which either the plane marked P, or that marked *b*, might be the terminal one. There is not any distinct cleavage that I can perceive parallel to either of these, or to the lateral planes, although there is some trace of it parallel to *b*. Some of the crystals have both their terminations complete.



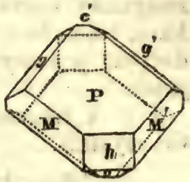
P on M.	128°	00′
P on T	140	50
P on b′	99	30
P on h	169	55
M on T.	117	6
T on k	133	20
b on M.	115	8
b on T	108	7

* It need scarcely be remarked that efflorescent salts should be measured when the air is moist, and deliquescent ones when it is warm and dry.

Chromate of Ammonia.

The primary form is *an oblique rhombic prism.* This may be cleaved in the direction of its two diagonals, but there is not any distinct cleavage parallel to the primary planes.

The planes P are frequently rounded, and the crystals are very thin.

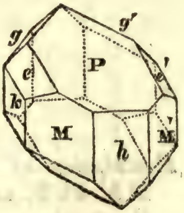


P on M, or M'.......	114°	00'
P on g...............	110	10
P on c'...............	101	58
P on *h*	122	31
M on M'...............	98	8
M on *h*...............	139	4
M on g...............	135	47

Chromate of Soda.

I have not perceived any distinct cleavage in these crystals, which effloresce so rapidly that the surfaces cease to reflect the images of objects almost before the planes can be measured.

The primary form is *an oblique rhombic prism,* the crystals being sometimes considerably lengthened in the direction of the horizontal diagonal.

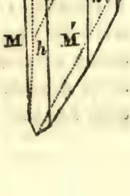


P on M, or M'.......	101°	16'
P on g...............	100	20
P on *e*...............	133	20
P on *h*...............	107	43
M on M'...............	80	4
M on *h*...............	130	8

Subcarbonate of Soda.

The primary form of this salt was given by Rome De L'Isle as *an octahedron with a rhombic base,* which form has been adopted by Haüy, and other writers, evidently without examining the crystals; for even with the common goniometer, the difference of more than 3° between the inclinations of M on M', and *e* on *e'*, of the annexed fig. 1, might have been readily detected.

Fig. 1.



On examining some large and bright crystals received from Mr. R. Howard, I observed that two of the four edges of the supposed base of the octahedron were replaced by narrow planes; and on looking through the crystals, I perceived indications of cleavages parallel to the edges that were not replaced. In this direction, they may be very easily cleaved, but I do not find that they yield to cleavage parallel to the replaced edge, or to any of the other

planes. On dissolving and recrystallising this salt, I obtained crystals resembling fig. 2, and others much more reduced in height; some of these are so thin as to leave scarcely a vestige of the planes M and *h*, and several are hemitropes, the plane of imaginary section being parallel to P. I have, therefore, been induced to consider the primary form *an oblique rhombic prism.*

Fig. 2.

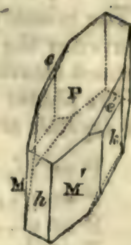


Fig. 1 represents the ordinary shape of the crystals.

P on M, or M′	108°	43′
P on *e*, or *e*′	129	52
P on *h*	121	20
M on M′	76	12
M on *h*	128	6
M on *k*	141	54
e on *e*′	79	44
e on *k*	140	8

Article X.

Description of the Galvanoscope. By the Rev. J. Cumming, MA. FRS. and Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy.*)

My dear Sir, *Cambridge, Sept.* 17, 1823.

I have found the galvanoscope, mentioned in the note of my last communication, so useful in detecting minute electromagnetic action, that I wish it to be more generally known than it seems to be at present; you will, therefore, oblige me by inserting an account of it in the next number of your *Annals.*

The drawing and description (Plate XXIII), are taken from the first volume of our Cambridge Transactions, with the addition of the mode of neutralizing the needle, which I find preferable to what I then proposed.

Its delicacy is such as to show a deviation of from 20° to 30° by the galvanic action of zinc and copper surfaces not exceeding 1-1600th of an inch. Disks of one inch diameter moistened with spring water, alcohol, or sulphuric ether, give nearly the same deviation. Two wires of silver and platina, each 1-100th inch diameter, and 3 inches long, twisted together at one end, and heated by a spirit lamp, gave a deviation of 90°.

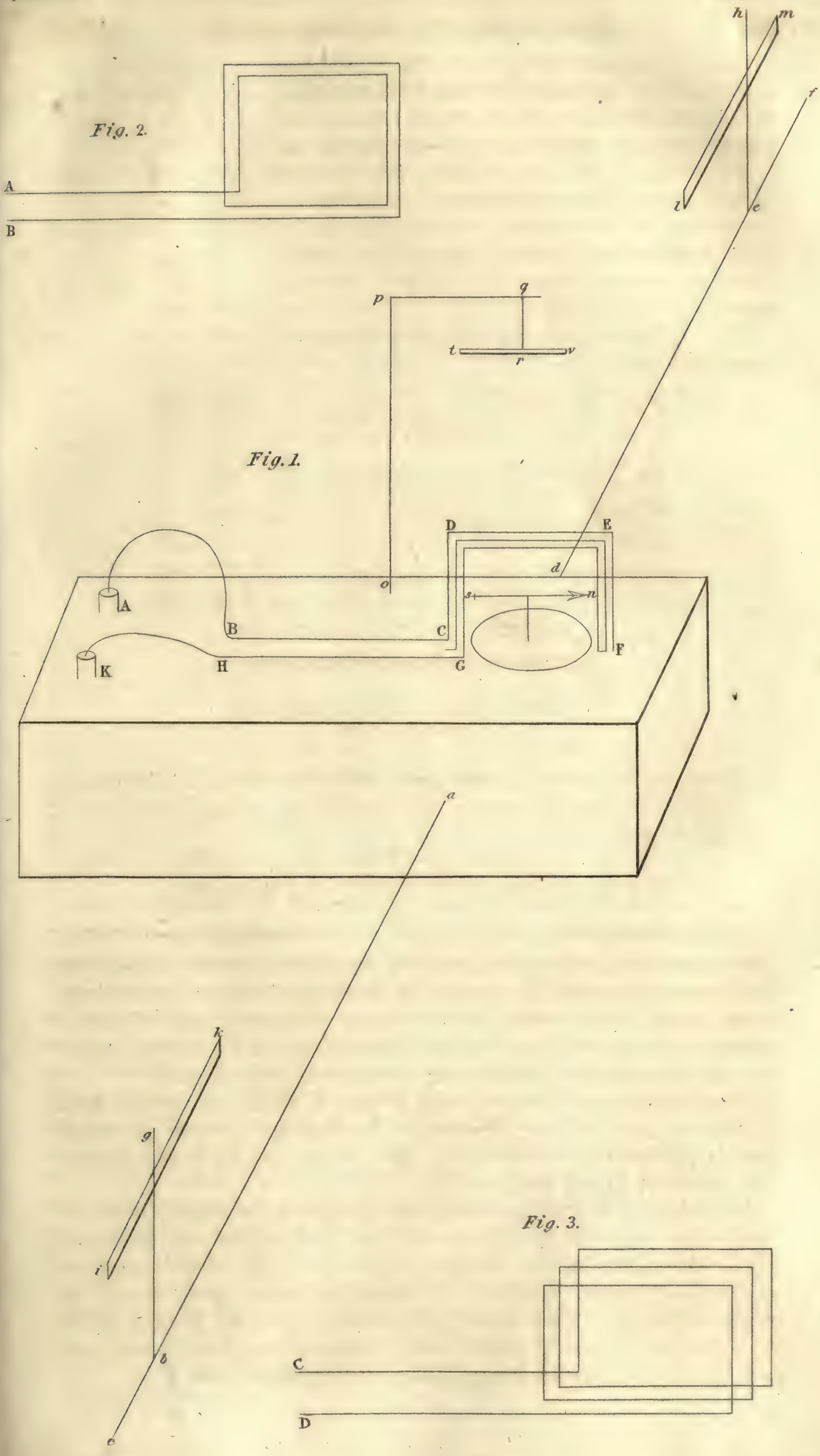


Fig. 2.

Fig. 1.

Fig. 3.

Fig. 1, the galvanoscope.

A K, tubes filled with mercury, to be connected with the galvanic plates.

A B C D E F G H K, a wire placed in a spiral form, round the compass needle *n s*.

a b c, d e f, brass wires inserted in the galvanoscope, and carrying the sliding wires *b g* and *e h*.

i k, l m, the neutralizing magnets attached to the wires *b g* and *e h*.

o p q r, a brass wire inserted in the galvanoscope at *o*, and carrying a small magnet moveable round *q r*.

The galvanoscope is placed east and west; the compass needle is then brought nearly into the plane of the spiral by the large magnets *i k, l m*, and the adjustment is completed by the small magnet *t v*.

It is desirable that the spiral wire should not be less than 1-25th of an inch, and that there should be as little space as possible between the spiral parallelogram and the compass needle.

There should be at least four or five revolutions in the spiral, of which the vertical form, fig. 2, seems preferable to the horizontal, fig. 3, as permitting a better view of the needle.

Article XI.

Remarks on M. Longchamp's Memoir on the Uncertainty of Chemical Analysis. By Richard Phillips, FRS. L. and E. &c.

In this paper* M. Longchamp has detailed a great number of experiments performed with the intention of ascertaining the cause of the uncertain results which he obtained in analysing sulphuric acid and sulphates, by means of barytic salts. The subject is one of unquestionable importance, and if the experiments detailed by M. Longchamp are accurate, his inferences are just, and chemical analysis is at an end. As, however, all statements which tend to envelope the sciences in uncertainty are productive of mischief, by discouraging their cultivation, I shall endeavour to show that the evils to be apprehended from M. Longchamp's experiments are merely imaginary; and without minutely examining all the details into which M. Longchamp has entered, I think it will appear from his statements respecting the action of the barytic salts upon sulphuric acid, that but little confidence can be placed in his results.

One hundred parts of sulphuric acid of specific gravity 1·812

* Annales de Chimie et de Physique, tom. xxiii. p. 155.

are stated to have given the following quantities of sulphate of barytes, when decomposed by means of the nitrate and muriate of barytes:

By the Nitrate.

Exper. 1	221·030
2	217·660
3	213·109
4	209·667
Giving a mean of	215·3665

By the Muriate.

Exper. 1	211·277
2	211·912
Giving a mean of.	211·5945

"Thus," says M. Longchamp, "100 parts of the same sulphuric acid gave

"By the nitrate of barytes. ...... 215·3665
By the muriate of barytes ...... 211·5945,"

and the inference which he deduces from these experiments is, that it is evidently impossible to determine the quantity of real acid which dilute sulphuric acid contains, by means of the salts of barytes.

A few observations will be sufficient to prove that this inference is unwarranted by the experiments which are supposed to prove its truth, and it will, I think, readily appear, that M. Longchamp's method of performing experiments is radically defective. The difference of the mean results, it will be observed, amounts to only 3·7720 parts, whereas the difference of two experiments with the nitrate of barytes, is 11·363 parts; it is, therefore, evident, that one or both of these experiments must be extremely erroneous. Again, if M. Longchamp had made only two experiments with the nitrate, as he has with the muriate, and those two had accidentally been the third and fourth stated, the mean would have been 211,·388 sufficiently approximating 211·5945, the mean of the two experiments with the muriate, to have entitled the author to have arrived at conclusions diametrically opposite to those which he has advanced.

In order, however, to put the subject to the test of experiment, I diluted some sulphuric acid with a considerable proportion of water, and divided the solution into eight parts. To four of these, solution of nitrate of barytes was added, slightly in excess, and the remaining four were similarly treated with muriate of barytes. The precipitates were washed with distilled water until sulphuric acid produced no effect in it: they were then all slowly dried at the same temperature until they ceased to lose weight.

The results were as follows, and I trust they will be considered as offering satisfactory evidence that similar results are obtainable by using either of the salts in question, and that they may be indifferently employed for the purpose of ascertaining the quantity of sulphuric acid.

Sulphate of Barytes by Nitrate.

Exper. 1	128·7 grs.
2	128·0
3	128·3
4	128·6
	513·6
Mean.	128·4

By Muriate of Barytes.

Exper. 1	128·1 grs.
2	128·7
3	128·0
4	128·5
	513·3
Mean.	128·325

Article XII.

Analyses of Books.

Transactions of the Linnean Society of London. Vol. XIV. Part I. 1823.

This part of the Linnean Transactions consists of eleven papers, of which the following are abstracts or analyses :—

I. *On the Malayan Species of Melastoma.* By William Jack, MD. Communicated by Robert Brown, Esq. FRS. and LS.

"The East Indian species of *Melastoma*," Dr. Jack observes, "have been little investigated in their native soil ; and the few that are mentioned in botanical works have for the most part been so imperfectly described as to occasion much confusion. This splendid genus has now become so extensive as to require being subdivided ; but to do this with due regard to the natural series, and to the relative importance of the characters, would demand a critical examination of the whole, and ampler means of reference than are accessible in India. I shall, therefore, confine myself to such observations as have been suggested by the Malayan species which I have had an opportunity of exa-

mining. The whole of these have baccate fruit, and are therefore true *Melastomæ,* as that genus is at present constituted. They vary much in the number of their stamina, but that number is constant in each species. They all agree in having the ovula attached to placentæ, which project from the inner angle of the cells; in the number of the cells corresponding with the divisions of the flower; in the peculiar inflexion of the anthers before expansion; and in having polyspermous berries. The points of difference to be principally attended to are the following: the similarity or dissimilarity of the alternate anthers; the number of the stamina; the anthers being with or without beaks; straight or arcuate; the calyces being hispid or nearly smooth, and having deciduous or persistent segments; the ovary being partially or completely adnate to the calyx. Of these characters, the only one which appears to me to point to a natural division of the species, is that of the equality or inequality of the stamina, occasioned by the anthers being alternately pedicellate and sessile on the filaments, as in *Melastoma Malabathrica,* or being all sessile, as in *M. exigua* and others here described. Those of the first division, with unequal stamina, have generally large and beautiful flowers, hispid calyces, with frequent deciduous segments, stamina always double the number of petals, which are either five or four, and arcuate rostrate anthers which, before the expansion of the flower, have their beaks lodged in cells betwixt the calyx and ovary. Those of the second division, with equal stamina, have seldom such conspicuous flowers, have smoother calyces, with segments generally persistent, eight stamina, rarely or never ten, and occasionally only four; anthers sometimes neither arcuate nor rostrate, and their points in that case do not reach before expansion below the summit of the ovary, which is then completely adnate to the calyx. The genus *Maieta* of Ventenat has been founded upon this latter character alone; but it is obviously insufficient for a generic distinction, as it can only be considered secondary to that of the relative length of the anthers, on which depends the complete or partial adhesion of the calyx and ovary; and a little attention to the relations of the different species to each other will show, that a division founded on this latter character could not be established without great violence to their natural affinities. The following species are arranged according to the division now suggested:"

We now present the specific characters, synonymes, and localities, of the various species of Melastoma described by Dr. Jack, necessarily omitting, as we must likewise do in similar cases throughout this article, his particular descriptions of the plants; but retaining a few important observations.

* Antheris alternis dissimilibus (Melastoma).

1. *Melastoma Obvoluta.* W. J.

M. decandra, foliis ovatis quinquenerviis appresso-pilosis,

floribus 3—5 terminalibus, bracteis magnis, calycibus, squamosis, laciniis ovatis deciduis. At Tappanooly on the west coast of Sumatra.

2. *Melastoma Malabathrica. Lin.*

M. decandra, foliis elliptico-lanceolatis quinquenerviis scabris, pilis brevibus appressis, floribus 7—11 opposite corymbosis, bracteis ovatis deciduis calyce minoribus, calycibus squamosis, laciniis deciduis. Kadali. *Rheed Malab.* iv. p. 87, t. 42. Fragarius niger. *Rumph. Amb.* iv. p. 137, t. 72. Sikadudu. *Malay.* Abundant throughout Sumatra and the Malay islands, and chiefly occupying open waste lands or coppices.

"In giving the above character of this well-known species," says Dr. Jack, "I have been obliged to add to the usual specific phrase, in order to distinguish it from the preceding, to which it has so much resemblance that they might easily be confounded together. The leaves of this are longer and less hairy, and the scales of the calyx are much shorter and more appressed than in *M. obvoluta.* The principal distinction, however, is in the inflorescence, the flowers in this being numerous, generally from seven to eleven, in a kind of corymbose panicle, and the bracts small; while in the preceding, the number of the flowers seldom exceeds three, and each is invested by two large bracts, which entirely inclose the calyx, and do not fall off till the petals are fallen. The two following species have also considerable resemblance to the present, but are readily distinguished on inspection by having their calyces covered with erect bristles in place of flat scales. This species (as well as all the rest) has the ovula attached to placentæ projecting from the inner angle of the cells: as the fruit ripens, the cells become filled with pulp, and the placentæ consequently less distinct: this probably occasioned Gærtner to fall into an error in ascribing to *Melastoma* nidulant seeds, and establishing on this a distinction between it and *Osbeckia.*"

3. *Melastoma Erecta.* W. J.

M. decandra, foliis quinquenerviis ovatis utrinque acutis villosis, floribus 5—7 terminalibus corymbosis, calycibus scabris pilis longis erectis, laciniis linearibus deciduis. Found at Tappanooly, in Sumatra.

4. *Melastoma Decemfida. Roxb.*

M. decandra, floribus subsolitariis terminalibus, foliis quinquenerviis, calyce decemfido setis mollibus porrectis echinato. *Roxb. Cat. Hort. Beng.* p. 90. Native of Pulo Penang.

5. *Melastoma Stellulata.* W. J.

M. octandra, pedunculis axillaribus 1—5 floris, calycibus setosis, setis erectis spinescentibus apice stellato—multifidis,

foliis oblongo-ovatis trinerviis subtus tormentosis. Daduruh Akkar. *Malay*. West coast of Sumatra.

"The peculiarity of the bristles of the calyx having stellate points, at once distinguishes this species from all the rest. Besides these bristles the calyx is covered with a short ferruginous wool, and the segments appear to be persistent. It was sent to me from Saloomah, and is by no means a common species."

6. *Melastoma Nemorosa*. W. J.

M. octandra, pedunculis axillaribus 1—3 floris, foliis ovato-lanceolatis quinquenerviis subtus cum calycibus, ramis, pedunculisque ferrugineo-villosis. Banga utan. *Malay*. Native of the Malay islands.

7. *Melastoma Bracteata*. W. J.

M. octandra, floribus paniculatis terminalibus, bracteis magnis ovatis, foliis cordato-ovatis quinquenerviis, calyce stellulato piloso, limbo subintegro. Oosa. *Malay*. Native of Pulo Penang.

** Antheris omnibus consimilibus. (*Stomandra*.)

8. *Melastoma Exigua*. W. J.

M. octandra, paniculis terminalibus, foliis longe petiolatis ovatis acuminatis glabris quinquenerviis, calyce quadridentato. Native of Pulo Penang.

"The fruit of this species might perhaps properly be considered a capsule, as it appears to be destitute of pulp. The gradations from a berry to a capsule in this family are such, that it is difficult to draw the line of distinction; and it seems questionable, whether this difference, when unsupported by other characters, can be considered of generic value."

9. *Melastoma Rotundifolia*. W. J.

M. octandra, foliis maximis subrotundis septemnerviis, floribus capitatis involucratis. Segoonil. *Malay*. Found in the Musi country, in the interior of Sumatra.

"This is a very singular and well-marked species, distinguished from all the others of the genus by its large subrotund leaves, and by the peculiarity of having the flowers in a crowded head surrounded by a large involucre. In this particular, it deviates widely from the usual habit of the *Melastomæ*. It is rarely met with, and has only been observed by me from Musi, a district lying immediately inland of Bencoolen."

10. *Melastoma Pallida*. W. J.

M. octandra, floribus paniculatis axillaribus et terminalibus, foliis ovatis quinquenerviis glabriusculis, antheris supra basin affixis. Native of the Malay islands.

11. *Melastoma Fallax.* W. J.

M. tetranda, paniculis terminalibus, foliis ovatis quinquenerviis subtus tormentosis, antheris erectis infra medium affixis. Native of Sumatra.

12. *Melastoma Gracilis.* W. J.

M. octandra, staminibus alternis nanis, paniculis terminalibus gracilibus, foliis ovatis acuminatis glabris trinerviis, ramis compressis. Sedudu akar. *Malay.* Sumatra.

13. *Melastoma Glauca.* W. J.

M. tetranda, paniculis terminalibus glaucis, foliis quinquenerviis acuminatis basi cordatis glabriusculis. Osbeckia tetranda. *Roxb. Cat. Hort. Beng.* p. 88. Tuniong utan. *Malay.* Abundant at Pulo Penang.

14. *Melastoma Viminalis.*

M. octandra, foliis oblongis obtuso-acuminatis basi cordatis quinquenerviis, paniculis trichotomis, bracteis oppositis oblongis ciliatis, antheris quatuor alternis sterilibus. Native of Sumatra.

15. *Melastoma Eximia.*

M. octandra, paniculis terminalibus, foliis maximis glaberrimis elliptico-ovatis quintuplinerviis. Found on the side of Gunong Bunko, commonly called the Sugar-loaf Mountain, in the interior of Bencoolen.

16. *Melastoma Rubicunda.* W. J.

M. octandra, floribus axillaribus dichotome cymosis rubescenti-pellucidis, calycis margine integro, foliis oblongo-ovatis triplinerviis glaberrimis. Native of the forests of Singapore.

17. *Melastoma Pulverulenta.* W. J.

M. octandra, floribus terminalibus corymboso-paniculatis rubicundis pulverulentis, foliis ovatis basi bituberculatis glaberrimis trinerviis. Sibiring. *Malay.* Found, along with the preceding, at Singapore, and in many parts of Sumatra, and the islands which skirt its western coast.

18. *Melastoma Alpestris.*

M. decandra, paniculis terminalibus, foliis sessilibus glaberrimis crenulatis quintuplinerviis. Found on the summit of the Sugar-loaf Mountain (Gunong Bunko), in Sumatra.

"This is the first decandrous species I have met with," Dr. Jack observes, "belonging to the second division of Melastomæ with similar anthers. In habit, and in the texture of the leaves, it has a close resemblance to *M. pulverulenta*, but its flowers have more resemblance to those of *M. rubicunda;* it must be associated with these two. From the characters of this species, it

appears that neither the number of the stamina, nor of the nerves of the leaves, afford subdivisions consonant to the natural series. I met with this plant on the very summit of the Sugar-loaf, along with *Rhododendra* and *Vaccinia.*"

A plate accompanies this communication, exhibiting the parts of fructification and the fruit of *Melastoma Malabathrica, M. exigua,* and *M. alpestris.*

II. *On Cyrtandraceæ, a new Natural Order of Plants.* By William Jack, MD. Communicated by Aylmer Bourke Lambert, Esq. FRS. VPLS.

Dr. Jack's introductory remarks in this paper are as follows:

"In examining some of the numerous Jumatran species of *Cyrtandra,* I was lately led to observe the great inaccuracy of Forster's description and figure of the fruit, which has been the cause of deception in regard to its natural affinities. His error consists in representing the septum as complete, with adnate placentæ similar to what obtains in some genera belonging to *Scrophularinæ;* whereas, in reality, it is bipartite through the axis of the fruit, and the placentæ are no other than the revolute lobes of the septa. This peculiar structure is more distinct in the nearly related genus of *Didymocarpus* (*Mal. Misc.* vol. i.), which has capsular fruit, and where the lobes of the contrary dissepiment so completely bipart the cells as to give it the appearance of being quadrilocular. It is obvious that this character is totally inconsistent with that of *Scrophularinæ,* and it does not accord exactly with any of the Jussiæan orders. *Didymocarpus* is related to *Bignoniaceæ* through *Incarvillea,* but it is not admissible into that family as defined by Mr. Brown in his *Prod. Fl. Nov. Holl.* I am therefore inclined to think that *Cyrtandra, Didymocarpus,* and another genus, which I shall here present under the name of *Loxonia,* which agree remarkably in general habit as well as in carpological structure, may properly form a small and distinct family near to *Bignoniaceæ.* The two first genera are numerous in the Malay islands; and I may remark that, as far as my present observations extend, the *Cyrtandræ* appear to prevail principally to the south of the equator, and the *Didymocarpi* on the north, where it has even been found, according to the observations of Dr. Wallich, to extend to the alpine regions of Nepal. I shall proceed to give the characters by which this family and its genera are distinguished, and shall add descriptions of all the species that I have as yet had an opportunity of examining."

Cyrtandraceæ.

Calyx monophyllus, divisus. *Corolla* monopetala, hypogyna, sæpius irregularis, 5 loba. *Stamina. Filamenta* 4, duo plerumque, nunc quatuor antherifera. *Antheræ* biloculares, per paria connexæ. Ovarium disco glanduloso cinctum, biloculare vel pseudo 4 loculare, polysporum. *Stylus* simplex. *Stigma*

bilamellosum v. bilobum. *Capsula* v. *Bacca* bilocularis, bivalvis, polysperma. *Dissepimenta* contraria, biloba, lobis revolutis seminiferis, loculos bipartientibus (inde pseudo 4 locularis). *Semina* nuda. Herbæ vel suffrutices. *Folia* simplicia, plerumque opposita, altero sæpe abortivo aut nano, exstipulata. *Inflorescentia* axillaris.

"In this family the flowers nearly resemble those of the *Bignoniaceæ*, but have most frequently only two fertile stamina, and rarely exhibit any trace of a fifth. In fruit they are abundantly distinct; and the herbaceous stems, simple leaves, and axillary inflorescence, form important and striking differences of habit."

CYRTANDRA, Forst.

Calyx quinquepartitus, *Corolla* infundibuliformis, ad faucem ampliatus, limbo quinquelobo subirregulari, rarius bilabiato. *Stamina* quatuor, quorum duo antherifera. *Bacca* oblonga, calyce longior; dissepimenti lobis per totam superficiem seminiferis. *Semina* nuda, sæpe foveolata v. punctata. *Folia opposita, altero plerumque abortivo aut nano. Flores sæpissime capitati involucrati.*

* Herbaceæ corollâ subirregulari.

1. *Cyrtandra Macrophylla.*

C. foliis subrotundo-ovatis serratis glabris, involucro monophyllo, pedunculis petiolo brevioribus. Selabang. *Malay.* Native of the interior of Sumatra.

2. *Cyrtandra Maculata.*

C. foliis subrotundo-cordatis acutis serratis supra glabis, corollæ lobis tribus inferioribus maculâ purpureâ. Sumatra.

3. *Cyrtandra Bicolor.*

C. foliis elliptico-lanceolatis basi cordatis supra glabris, subtus villosis purpureis, pedunculis petiolo brevioribus. Sumatra.

4. *Cyrtandra Hirsuta.*

C. foliis elliptico-ovatis basi cordatis crenatis utrinque pilosis, capitulis paucifloris hirsutis, involucro bipartito. Sumatra.

5. *Cyrtandra Glabra.*

C. foliis lato-ovatis serratis glabris, capitulis breve-pedunculatis, involucro monophyllo. Interior of Bencoolen.

6. *Cyrtandra Incompta.*

C. hirsuta, foliis elliptico-ovatis serratis, floribus capitatis hirsutis, involucro diphyllo. Langkabang. *Malay.* Native of Sumatra.

7. *Cyrtandra Aurea.*

C. foliis oppositis subrotundo-ovatis acuminatis serratis sericeo-pilosis, capitulis densis subsessilibus. At the foot of Gunong Bunko, interior of Bencoolen.

8. *Cyrtandra Peltata.*

C. foliis peltatis ovatis acuminatis. Sumatra.

9. *Cyrtandra Carnosa.*

C. foliis lanceolato-oblongis basi obliquis carnosis oppositis, altero minimo subrotundo.

** Frutescentes, corollâ bilabiatâ.

10. *Cyrtandra Frutescens.*

C. erecta, foliis oppositis lanceolatis serratis glabris, pedunculis axillaribus trifloris.

"This species and the following differ considerably in habit from the other *Cyrtandræ*, and have more resemblance to *Didymocarpus frutescens;* from which, however, they are distinguished by their baccate fruit, and by the insertion of the seeds upon the whole surface of the lobes of the dissepiment; while in *Didymocarpus* they are attached only to the edge. These species might perhaps be separated from *Cyrtandra* on account of their bilabiate corolla and larger fruit."

11. *Cyrtandra Rubiginosa.*

C. erecta, foliis obovato-lanceolatis serratis, pedunculis axillaribus fasciculatis unifloris, cum calycibus viscoso-pilosis.

Didymocarpus. Wallich.

Calyx 5 fidus. *Corolla* infundibuliformis, limbo quinquelobo, sub irregulari, rarius bilabiato. *Stamina* 4, rarissime 5, quorum duo nunc quatuor antherifera. *Capsula* siliquæ-formis, pseudo-quadrilocularis, bivalvis, hinc dehiscens; dissepimenti contrarii lobis valvulis parallelis iis denique æmulis (ideoque fructum bicapsularem mentientibus) margine involuto seminiferis. *Semina* nuda pendula.

Folia simplicia opposita, raro alterna, æqualia, floribus axillaribus pedunculatis vel racemosis.

1. *Didymocarpus Crinita. Malay Miscell.* vol. i.

D. erecta, foliis alternis longis spathulatis acutis serratis pilosis subtus rubris, pedunculis 2—5 axillaribus unifloris basi cum petiolis coeuntibus. Timmu. *Malay.* In the forests of Pulo Penang.

2. *Didymocarpus Racemosa.*

D. foliis oppositis lanceolatis utrinque attenuatis duplicato-

serratis supra glabris, pedunculis axillaribus plerumque bifidis, floribus racemosis, pedicellis binatis. At Tappanooly, on the west coast of Sumatra.

3. *Didymocarpus Reptans. Mal. Misc.* vol. i.

D. prostrata reptans, foliis petiolatis ellipticis crenulatis, pedunculis 1—3 axillaribus unifloris, staminibus duobus fertilibus. Timmu Kichil. *Malay.* Found in the forests of Pulo Penang with the preceding.

4. *Didymocarpus Corniculata. Mal. Misc.* vol. i.

D. erecta, foliis alternatis obovatis acuminatis serratis, floribus fastigiatis secundis, pedunculo axillari elongato. Found at Tappanooly, in Sumatra.

5. *Didymocarpus Elongata.*

D. herbacea erectiuscula didynama, foliis oppositis ovatis utrinque acutis serratis, spicis axillaribus secundis, pedicellis binatis remotis, corollâ elongatâ. Found on Pulo Bintangor, an island lying off the west coast of Sumatra.

6. *Didymocarpus Barbata.*

D. fruticosa, foliis oppositis ovatis subinæquilateralibus hirsutis, pedunculis gracilibus axillaribus fasciculatis 2—6 floris, staminibus quatuor apicebarbatis: duobus sterilibus, calyce infundibuliformi. Native of Sumatra.

7. *Didymocarpus Frutescens. Mal. Misc.* vol. i.

D. caule suffrutescente erecto, foliis oppositis longe petiolatis ovato-lanceolatis utrinque attenuatis supra glabris subtus canescentibus, floribus axillaribus fasciculatis didynamis. Native of Pulo Penang.

LOXONIA.

Calyx 5 partitus. *Corolla* infundibuliformis, limbo quinquefido bilabiato. *Stamina* quatuor fertilia, corollâ breviora. *Stigma* bilobum. *Capsula ?* ovata, calyce inclusa, bilocularis, polysperma; dissepimenti contrarii lobis revolutis seminiferis. *Semina* nuda. *Foliis oppositis altero nano, plerumque inæquilateralibus, floribus racemosis.*

1. *Loxonia Discolor.*

L. foliis supra glabris, subtus retrorsum scabris purpurascentibus, racemis simplicibus elongatis. Found in the interior of Bencoolen.

2. *Loxonia Hirsuta.*

L. hirsuta, foliis semiovatis latis, pedunculis 2—4 fidis, floribus racemosis. Native of Sumatra, interior of Bencoolen.

Æschynanthus.

Calyx ventricoso-tubulosus, 5 fidus. *Corolla* limbo subirregulari. *Stamina* 4 antherifera, exserta, sæpius rudimento quinti, *Capsula* longissima, siliquæformis, bivalvis, pseudo 4-locularis, seminibus numerosis (aristatis). *Suffrutices debiles, foliis carnosis, floribus coccineis.*

The capsules of this genus nearly resemble those of *Didymocarpus*, and exhibit with great distinctness the peculiar character of this family. The seeds are attached to the whole of the inner surface of the lobes, and are singular in being awned. The exsert stamina and crimson flowers are further deviations from the usual habit of its congeners.

1. *Æschynanthus Volubilis.*

A. caule volubili, calycibus glabris. Found in the neighbourhood of Bencoolen.

2. *Æschynanthus Radicans.*

A. caule radicante, calycibus villosis. Simbar burong. *Malay*. Found in the forests of the interior of Sumatra growing on the trunks of old trees, with its root sometimes on the ground, sometimes on the tree.

This paper is illustrated with an engraving, showing the parts of fructification and the fruit of *Cyrtandra macrophylla*, *Didymocarpus crinita*, and *Æschynanthus volubilis.*

III. *Remarks on the Identity of certain general Laws which have been lately observed to regulate the natural Distribution of Insects and Fungi.* By W. S. Mac Leay, Esq. MA. FLS.

This singularly interesting and important paper will appear in the next number of the *Annals.*

IV. *Some Particulars of the Natural History of Fishes found in Cornwall.* By Mr. Jonathan Couch. Communicated by Sir James Edward Smith, MD. FRS. Pres. LS.

Mr. Couch, it appears, had intended to submit to the public attention, in a distinct work, the results of his icthyological researches in Cornwall, but that design having hitherto been frustrated, he has communicated the present sketch to the Linnean Society. We proceed to give the names of the fishes which are mentioned in it, with some of the more curious observations on certain species.

Apodal Fishes: *Muræna Anguilla,* Eel;—" The eel may be considered as a migratory fish. The young ones as soon as they are produced (which in the sphere of my observation is always within the reach of the tide) begin to advance up the river; and to accomplish this object, overcome difficulties of an extraordinary kind. I have seen them, at the fall of a river, dive below the moss, that hung from above into the water, and *worm* themselves upward through the fibres by the side of the stream, resting at intervals as if to recover strength;

and at last, when at the top, exert their utmost activity to stem the rapid current and reach a place of safety. In getting up the little cataract that pours over a sloping rock, they prefer those places which are only moistened by the droppings from above; but those which quit the moisture altogether, as I have seen some do, are obliged to alter their course, and proceed to places more easy for them to travel in. The motive for this migration, which is general among young eels, I have not been able to discover. Some among them I have noticed to be so diaphanous that the vertebræ may be counted; and taking advantage of an opportunity of this kind, I ascertained that when in a state of activity, and not alarmed, the pulsations of the heart were 40 in a minute."—*Murœna Conger,* conger; *Xiphias gladius,* swordfish; *Ammodytes tobianus,* launce.

Jugular Fishes: *Callionymus Lyra,* Dragon fish; *C. Dracunculus,* Skulpin; Mr. Couch gives the common English name of this fish, because he in general prefers it to that which is arbitrarily bestowed by naturalists:—*Trachinus Draco,* greater weever: "I have known such effects to arise from the puncture of the spine on the gill-covers of this fish," Mr. Couch remarks, "as can only be accounted for on the supposition of its conveying some venomous quality. In three men who were wounded by one fish, the pain and tension proceeded from the hand to the shoulder in a few minutes." *Gadus Æglefinus,* haddock; *G. Morhua,* cod; *G. Luscus,* bib; *G. minutus,* poor; *G. Molva,* ling; *G. Mustela,* rock-ling; "The variety of this fish which possesses five barbs, has been supposed to be a distinct species; but from attentive consideration I am convinced that this is a mistake:" *G. Merlangus,* whiting; *G. Pollachius,* whiting pollack; *G. Carbonarius,* rauning (ravening) pollack, or coal-fish; *G. Merlucius, lake.*—*Blennius Pholis,* shanny; *B. galerita,* crested blenny; *B. Gunellus,* butterfish; *B. Phycis,* greater forked beard; "I would suggest that this fish might with propriety be placed in a genus, which might be denominated *Phycis;* and be distinguished by the barb at the throat:"—Lesser Forked Hake.—The insertion of this species is on the authority of Mr. Jago in Ray's *Synopsis;* as I have never had the good fortune to meet with a specimen.*

Thoracic Fishes: *Cepola rubescens,* red snakefish. "Two specimens of this fish have come into my possession; one of

* "Since this paper was read, I have met with the *Lesser Forked Beard* of Jago; length ten inches; head wide and flat; eyes forward and prominent; under-jaw shortest; teeth in the jaws and palate, sharp and incurved, and some in the throat; small barb at the under jaw; body compressed, smooth; first dorsal fin triangular and extremely small; second dorsal fin and the anal fin long, ending in a point; tail round; ventral fins have several rays, of which the two outmost are much elongated, the longest measuring two inches; the fins all covered with the common skin; a furrow passes above the eye to the back; stomach firm, with longitudinal folds; no appendix to the intestines; air-bladder large, and of unusual form. In the intestines were the remains of an *Echinus.* This fish has all the marks of a *Gadus,* to which genus it appears to me properly to belong.—J. C."

them, about five or six inches in length, was taken with a line; the other, from which my description was taken, was thrown on shore in a storm. It measured fifteen inches in length, an inch and a quarter in depth of the deepest part, including the dorsal and anal fins, and was very thin; but the smaller specimen above alluded to was nearly round. It tapered both in depth and thickness toward the tail. The angle of the mouth was much depressed, which caused the under jaw to appear the longest; both were armed with long and sharp teeth. The eyes were large, and the head short before them. The dorsal fin was twelve inches in length, and had seventy rays; the anal fin was eleven inches long, and had sixty rays; the tail distinct, spear-shaped, of twelve rays, the middle rays being two inches long, and ending in a point, and the rays at the sides not exceeding a fourth of that length. The ventral fins were pointed, and fastened to the body for about half their length by a fine membrane. Beside the lateral line there was a row of small bony prominences near the dorsal fin. The colour was a diluted red. From the inspection of several specimens, I am inclined to think that this ought to be ranked as a Jugular Fish."

Gymnetrus Hawkenii, Bloch, ceilconin; *Gobius Aphya*, spotted goby; *G. niger*, rockfish; *Cottus gobio*, bull-head; *Zeus Faber*, doree: *Pleuronectes Hippoglossus*, holibut; *P. rhomboides*, kite; *P. punctatus*, whiff; *P. Rhombus*, pearl; *P. megastoma?* Don. carter, or lanternfish.

Chætodon.—"Only one species of this genus has come within my notice. This was taken at Looe, swimming alive on the surface of the water, in August, 1821; and as I have not been able to refer it to any described species, I subjoin a description.

"It was about seventeen inches long, and, exclusive of the dorsal fin, five inches and a half deep; the snout was blunt, sloping suddenly above the eyes; the angle of the mouth depressed; the teeth, numerous, sharp, incurved, four in front of the under jaw very long; the body deep, thin; two dorsal fins, the first having flexible rays, the second long and narrow; tail very deeply lunated; the pectorals long; the ventrals double, or having a wing, by which means it seemed to have four ventral fins; the anal fleshy, and somewhat expanded at the origin, obscure in its progress towards the tail; no lateral line; a broad band from eye to eye; the colour blue, deeper on the back than on the belly; covered with large scales, as well the body as the fins, so that the dorsals and anals seem like an extension of the body. I was unable to count the rays of the dorsal fins."

Sparus Smaris, bream; *S. Pagrus*, becker; *S. Vetula, C.* oldwife:—"Although the English name here given to a species of *Sparus* is applied by naturalists to one of a different genus, yet I am obliged to use it to designate a fish presently to be described, as it is the only one which our fishermen make use of. The body is deep, compressed, and has a considerable

resemblance to the *S. Pagrus;* the lips are fleshy, and the jaws furnished with a pavement of teeth, of which those in front are the longest; the gill-membrane has five rays; the gill-covers and body are covered with large scales. The ten first rays of the dorsal fin are spinous; the anal fin also has four spinous rays, after which it becomes more expanded; the tail is concave. This fish has a membranous septum across the palate, as in the Wrasse genus. When in high season, the colour behind the head is a fine green; towards the tail it is a reddish orange; the belly has a lighter tinge of the same colour. When out of season, the whole is a dusky-lead colour. It weighs about three pounds."

Labrus Tinca, common wrasse; *L. bimaculatus,* bimaculated wrasse; *L. Coquus,* cook:—"The habits of this species and of *L. comber* are similar. In the summer they are found near the shore; in winter they pass into deeper water; but are taken by fishermen through the year, and are principally employed as bait for other fish.

"Besides these and *L. cornubiensis,* I have noticed another species, which is by fishermen confounded with the *L. Tinca,* and which I am unable to refer to any Linnæan species. It differs from the common wrasse in the following particulars: —The body is longer in proportion to its depth, and somewhat thicker; the ventral fins, which in the *L. Tinca* reach just to the anus, in this reach but two-thirds of that distance; a light-coloured line runs from the eye to the tail; the anterior bone of the gill-cover has a smooth margin, but in the *L. Tinca* it is finely serrated; the lateral line also forms an acute angle at its curve, pointing downwards in the *Tinca;* in this species it has a gentle curvature; it has twenty spinous rays in the dorsal fin. The colour of the back is a dark-brown, lighter at the sides, saffron-coloured on the belly. It is common.*"

Sciæna Labrax, basse;—Stone Basse; *Gasterosteus Ductor,* pilotfish;—"two of this species, a few years since, accompanied a ship from the Mediterranean into Falmouth, and were taken in a net;" *Scomber Scomber,* mackerel; *S. Trachurus,* scad; *S. glaucus,* albacore; *Mullus Surmuletus,* striped surmullet; *Trigla Lyra,* piper; *T. Cuculus,* Elleck; *T. Gurnardus,* grey gurnard.

Abdominal Fishes: *Salmo Salar,* salmon; *S. Trutta,* salmon trout; *S. salmulus,* palmer trout; *S. Fario,* common trout or shote; *Esox Belone,* garpike;—"the intestinal canal of this fish runs straight from the gullet to the anus, without any appendix or convolution, or distinction between the stomach and the bowels."

E. Saurus, skipper.—"This species does not take a bait. A native of the same climate, this fish nearly resembles the flying-fish in its manners and its fate. Frequently, when the weather

* "This appears to be a variety of *Labrus Julis.*"

is fair, they are seen to spring from the bosom of the deep, pass over a space of thirty or forty feet, and plunge into the water to rise again in a moment, and flit over the same distance. Sometimes this may proceed from wantonness, and sometimes probably from an impulse to escape from the voracious inhabitants of the deep; but it seems surprising that a fish so scantily provided with fins should be able to make such an extraordinary leap; for the pectoral fins, instead of reaching nearly to the tail, as in the flying-fish, are very small; and though well adapted by their figure to raise and direct the head, cannot afford assistance in supporting the body in the air. The whole motion is effected by the action of the tail and finlets alone, and is more properly a leap than a flight. This is a most excellent fish for the table."

E. Sphyræna, sea pike; "Besides these I have met with a species which I have never seen described, unless it be the *Esox Brasiliensis*, Linn. *Syst. Nat.* It was taken by me in the harbour at Polperro, in July, 1818, as it was swimming with agility near the surface of the water. It was about an inch in length, the head somewhat flattened at the top, the upper jaw short and pointed, the inferior much protruded, being at least as long as from the extremity of the upper jaw to the back part of the gill-covers. The mouth opened obliquely downward; but that part of the under jaw which protruded beyond the extremity of the upper, passed straight forward in a right line with the top of the head. The body was compressed, lengthened, and resembled that of the garpike, *E. Belone*: it had one dorsal and one anal fin placed far behind, and opposite to each other; the tail was straight. The colour of the back was a bluish-green, with a few spots; the belly silvery."

Mugil Cephalus, grey mullet; *Clupea Harengus*, herring; *C. pilchardus*, pilchard; *C. Alosa*, shad, alewife of the west; *C. Sprattus*, sprat; *Cyprinus Leuciscus*, dace;—it is doubtful whether this fish be an original native of Cornwall.

Branchiostegous Fishes: *Cyclopterus Lumpus*, lumpfish; *C. Cornubiensis*, Jura Sucker;—"I have seen two varieties of this fish, if they were not distinct species: in one the snout is shaped like a spatula; in the other, it was shorter, and ended in a point. The body and head are wide and depressed, with the eyes at the sides, and before each a double fleshy process, about the tenth of an inch long, in a fish that measured two inches; there is a fleshy tubercle close behind these processes. The lips membranous, the lower jaw a little the shortest, opening with a very wide gape. Behind the head are two dark spots, each with a bluish speck in the middle. The body tapers to the tail; the dorsal and anal fins begin at a third of the whole length from the tail, and run back to that part; the pectorals are far behind; the tail round. The sucking apparatus is formed of two circles, one before the other, furnished with numerous very small tubercles. The colour is dusky, sometimes crimson;

the belly flesh-coloured. When the colours faded after death, I observed many spots on the sides, which were not visible before. It adheres with some degree of force. When the tide retires, this fish sometimes takes refuge under a stone.

"Another species, which I do not recollect to have seen noticed, is not uncommon about low-water mark, where it hides under stones. The head is broad and flat, sloping from behind the eyes to the mouth. The body tapers from the pectoral fins to the tail; it is smooth, a dusky-yellow on the back and sides, the belly white; it has a row of white points along the lateral line, and also about the head and mouth, which secrete mucus. Thirteen tubercles form the sucking apparatus; but I could never get this fish to adhere to any substance. The tail is round; the dorsal and anal fins long, the former beginning just above the pectoral fins, the latter at the abdominal tubercles, and both run to the tail; which part, with the dorsal and anal fins, is crossed by dark bars. When this fish rests, it has a singular custom of throwing its tail forwards towards the head. It rarely exceeds an inch in length."*

Tetraodon truncatus, oblong sunfish; *Centriscus Scolopax*, trumpetfish:—"A fish of this species was thrown on shore in St. Austel Bay, and came into the possession of William Rashleigh, Esq. of Menabilly, a gentleman distinguished for his love of natural history, who possesses a fine drawing of it. It was five inches long, and from the back to the belly one inch and two-eighths; in thickness three-eighths of an inch; it weighed three drams. The proboscis, which to the eye measured an inch and five-eighths, was formed of a bony substance, which was continued along the back, where it terminated in a sharp point, and spreading in the middle, where it makes an obtuse angle just above a small fin behind the gills."

Chondropterygious Fishes: *Raia Torpedo*, torpedo or cramp ray:—Mr. C.'s suggestion respecting the use of the electrical faculty of this animal has already been given in the *Annals*, at p. 156 of the present volume.

Squalus Squatina, Monkfish:—"Common; keeps near the bottom, and is most commonly taken in nets. The propriety of ranking this fish with the *Squali* seems to me to be doubtful: the terminal mouth and depressed body afford sufficient distinctions for a new genus, which might be denominated *Squatina*, and in which the following species might find a place.

"Lewis.—This fish, so named by fishermen, by whom it is not unfrequently taken with a line, bears some resemblance to the Monk, but is somewhat smaller; and as I have not been able to assign it a Linnæan name, I subjoin a description:—The head is large, flat, the jaws of equal length, forming a wide mouth; the upper jaw falls in somewhat at the middle, so that

* "This is probably a variety of *C. liparis*."

at this part the lower jaw seems a little the longest; both are armed with several rows of sharp teeth; the tongue is small. The head is joined to the body by something which resembles a neck; the body is flat so far back as the ventral fins, beyond these it is round; the pectoral and ventral fins are very large; the former are flat, and both have near their extremities a number of spines. The two dorsal fins are placed far behind; the lobes of the tail are equal and lunated. There are five spiracula; the eyes are very small, and the nictitating membrane, which is of the colour of the common skin, contracts over the eye, leaving a linear pupil. The body is slightly rough, of a sandy-brown colour; the under parts white. It is about five feet long, and keeps near the bottom."

Squalus galeus, tope; *S. Mustelus,* smooth hound; *S. maximus,* basking shark; *S. cornubicus,* porbeagle.

"There are in the possession of William Rashleigh, Esq. of Menabilly, a drawing and memorandum of a fish of this genus, which I am not able to refer to any known species; it was twenty-nine feet four inches long, twenty-four feet round, the fork of the tail seven feet, and the weight four tons; in the drawing, the eye is in front, under a snout that projects and is turned upward; the mouth is two feet and a half wide. The head is deep; the first dorsal fin much elevated. This fish seems to resemble the basking shark, but differs from it in the form of the head and situation of the eye."

Accipenser Sturio, common sturgeon.

V. *A Description of some Insects which appear to exemplify Mr. William S. Mac Leay's Doctrine of Affinity and Analogy.* By the Rev. William Kirby, MA. FRS. and LS.

Intending, as before mentioned, to give Mr. Mac Leay's paper in our next, we purpose appending to it an abstract of the present communication.

VI. *Some Account of a new Species of Eulophus Geoffroy.* By the Same.

"*Eulophus Damicornis.* Aureo-viridis: abdomine nigricanti, basi macula pallida sub-pellucida. Long. corp. lin $1\frac{1}{4}$. *Habitat* in larva *Bombycis camelinæ?* Mus. nostr."

"This species," Mr. Kirby observes, "is very similar to *E. ramicornis* (of which, as well as of *C. pectinicornis,* I possess British specimens), the principal distinction being the white spot in the base of the abdomen." B.

(*To be concluded in our next.*)

Philosophical Transactions of the Royal Society of London, for 1823. *Part I.*

(*Concluded from* p. 227.)

IX. *On some Fossil Bones discovered in Caverns in the Limestone Quarries of Oreston.* By Joseph Whidbey, Esq. FRS. In a Letter addressed to John Barrow, Esq. FRS. To which is added, *A Description of the Bones,* by Mr. William Clift, Conservator of the Museum of the College of Surgeons.—(See *Annals,* v. 233.)

When adverting to the rarity of appearances of disease or fracture in fossil bones, in reference to such appearances in some of those from Oreston, as described in our report of this paper, Mr. Clift remarks, "On mentioning this circumstance to Prof. Buckland, he informed me, that he had lately seen in the collection of Prof. Sömmerring, of Munich, the skull of a very old hyæna from the caves of Gaylenreuth, in which the incisor and canine teeth, with the jaw containing them, had been entirely torn away, and the occipital and parietal crest dreadfully fractured and perforated, apparently in an affray with some more powerful animal; after which a healing and partial renovation of the parts had taken place, and the animal had lived on to mature old age, from the state of its masticating organs."

"Of the bovine genus," among the bones described by Mr. Clift, "there are specimens of the bony core of the horns belonging to three individuals of different size; all of them remarkably short, conical, and slightly curved, and standing in a nearly horizontal direction from the head. They evidently do not belong to very young animals, and from the appearance of these alone, a very small species would be inferred; but numerous specimens of the teeth, of the os humeri, ulna and radius, os femoris, tibia, os calcis, metacarpus and metatarsus, and phalanges, clearly prove that they belonged to individuals considerably larger than the average size of animals of that genus at the present day.

"The number of bones collected, afford sufficient grounds for supposing them to have belonged to more than a dozen individuals, varying considerably in their age."

The bones and teeth of five or six hyænas which formed part of this remarkable collection, have already been mentioned in the *Annals.* "But there are likewise detached specimens of the canine teeth, and molares of individuals of very large size; and the posterior part of a skull of uncommon magnitude, which corresponds most exactly in form with that of a hyæna, and must undoubtedly have belonged to that animal, but measures twice as much from every determinate point to another, as a recent full grown hyæna's skull."

"Since the above was written, Mr. Whidbey has transmitted

some additional specimens of the jaws and teeth of the hyæna, the wolf, and the fox, which have been subsequently discovered in one of the caverns, from which cavity all the bones of the wolf have been derived. Among these is half of the lower jaw of a hyæna of very superior magnitude to any of those previously discovered, and probably has belonged to the large skull before mentioned.

"The jaws of the wolf are of similar dimension with those before described; but one of them belonged to a very aged individual.

"Of the fox, there have been found only a few vertebræ, and two canine teeth from the lower jaw, which correspond perfectly in size and form with those of a recent animal; but are equally fragile and absorbent with those of the other animals."

Two engraved sketches of the caverns are annexed to Mr. Whidbey's account of them; and Mr. Clift's description of the bones is illustrated with five engravings, from drawings by himself.

X. *On the Chinese Year*. By J. F. Davis, Esq. FRS.—(See *Annals*, v. 149.)

"The Chinese year, properly considered as such, is in fact a lunar year, consisting of twelve months of twenty-nine and thirty days alternately, with the triennial intercalation of a thirteenth month, to make it correspond more nearly with the sun's course.* It has not been discovered (with any degree of certainty), *why* they fix upon the 15th degree of Aquarius as a rule for regulating the commencement of their lunar year; but they have an annual festival about the recurrence of this period which bears a considerable resemblance to the deification of the bull Apis; and this resemblance is increased by the connexion of both ceremonies with the labours of agriculture, and with the hopes of an abundant season. This coincidence may serve to fortify the opinions of those who are fond of tracing the Chinese to the Egyptians; although the possibility of such a derivation has been fully disproved by M. de Pauw."

XI. *Experiments for ascertaining the Velocity of Sound at Madras, in the East Indies.* By John Goldingham, Esq. FRS.

Mr. Goldingham's account of the manner in which these experiments were conducted, and of their general results, will be found in the last number of the *Annals*, p. 201.

XII. *On the double Organs of Generation of the Lamprey, the Conger Eel, the common Eel, the Barnacle, and Earth Worm, which impregnate themselves; though the last from copulating, appear mutually to impregnate one another.* By Sir Everard Home, Bart. VPRS.—(See *Annals*, v. 302.)

* "I call this intercalation triennial," Mr. Davis remarks, "because that is the nearest approximation; but in fact it is seven times in nineteen years."

The mean results of the Meteorological Journal kept at the Society's apartments, for the year 1822, are as follows: height of the barometer 29·863 inches, of Six's thermometer 55°; rain 18·068 inches. B.

Article XIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Medical and Scientific Instruction at Guy's and St. Thomas's Hospitals, Southwark.*

The Annual Course of Medical and Scientific Instruction at these Hospitals will commence early in the ensuing month of October, when distinct Courses of Lectures will be delivered on the following subjects, viz. Practice of Medicine; Pathology; Therapeutics and Materia Medica, by Drs. Cholmeley and Back, Physicians to Guy's Hospital.

Principles and Practice of Chemistry, by William Allen, Esq. FRS. Dr. Bostock, FRS. and Arthur Aikin, Esq. FLS.

Experimental Philosophy, by William Allen, Esq. FRS. and John Millington, Esq. Prof. Mech. Phil. Roy. Inst.

Midwifery and Diseases of Women and Children; and Physiology, by Dr. Blundell.

Anatomy and the Practice of Surgery, by Sir Astley Cooper, Bart. and Mr. Green.

Structure and Diseases of the Teeth, by Mr. Thomas Bell, FLS.

Medical and Practical Botany, by Dr. Bright.

A Course of Clinical Lectures will be delivered in the season.

Particulars to be had of Mr. Stocker, Apothecary to Guy's Hospital, who enters Pupils to all the above Lectures.

II. *Change in the Freezing Point of Thermometers.*

The following observations on this subject, a notice on which has already appeared in the *Annals* (for July, p. 74), are extracted from Mr. Daniell's newly published Meteorological Essays, p. 368.

"With respect to the change in the freezing point, which takes place in time in the best thermometers, I have lately had an unexceptionable opportunity of confirming the assertions of the French and Italian philosophers. Mr. Jones has obligingly put into my hands two thermometers of the late Mr. Cavendish, which have evidently been constructed with much care. The mercury in the balls of both flows freely into the tubes when reversed; and when suffered to fall sharply, strikes the ends with a metallic sound. The same *click* may be heard in the bulbs, when it is permitted to fall back, and the cavity closes without the slightest speck. These indications of a well-boiled tube are rarely to be met with in the common thermometers of the present day. They are mounted upon common deal sticks, and the graduation, which is only continued for a few degrees about the freezing point, is engraved upon a small slip of brass. The degrees are very

large, and they are distinctly divided into tenths. Each degree of No. 1 occupies a space of ·208 inch, and of No. 2 ·130 inch. The scratch upon the glass for the freezing point is very visible in both. It is difficult to say for what purpose they were originally made, but evidently for some experiments upon the freezing point of water; and if they had been expressly constructed to verify the present point, they could not have been better contrived for the purpose. The bulbs of both were plunged into pounded ice, in which they were left for half an hour, and the height of the mercury was carefully taken by two observers with the aid of magnifying glasses. The result of the examination was, that in No. 1 the freezing point upon the scale was 0·4 degree too low, and in No. 2, 0·35 degree. There can be little doubt, I think, that the right cause of the phenomenon has been assigned, *viz.* the change of form and capacity which the glass undergoes from the pressure of the atmosphere upon the *vacuum* of the tube."

III. *Notice in regard to the Temperature of Mines.* By Matthew Miller, Esq. MWS.

The late experiments on the temperature of mines made in Cornwall, and in other countries, having given rise to various speculations in regard to the distribution of heat in the crust of the earth, all of which appear to me to be unsatisfactory, I now beg leave to offer for consideration of the Society, an explanation, that does not seem liable to the objections that have been opposed to the others.

In every mine, with the exception of a few, which are level-free, the ventilation is carried on by causing the air at the surface to descend, and traverse the works, and then ascend. Now it is evident, that if a portion of air from the surface be carried down to the bottom of the mine, it will be condensed in proportion to the depth of the mine; and, in consequence of this condensation, will become heated, and the degree of heat will of course be in proportion to the depth of the mine. The air thus heated, traverses the works, and imparts its heat to the strata; it then ascends, and is succeeded by a fresh portion of air from the surface, which in the same way becomes heated, and imparts its heat to the strata, and they, in turn, communicate it all around. Thus in a long course of working in a deep mine, the air at the bottom is heated, and also the rocks to a considerable depth; and when the working ceases, the mine takes a long time to lose its temperature; and this is found to be the case, particularly when the mine becomes full of water, the water being found at first of a high temperature, and gradually to lose its heat, which is in consequence of the strata imparting theirs to the water, and as soon as they have given out all their heat, the water indicates the mean temperature nearly of the place.

The reverse takes place in an old mine when re-worked; in that case, the temperature rises gradually as the working continues; and in those mines which are not worked, but in which the ventilation still goes on, I believe it will be found that they do not lose more of their temperature than can be placed to the abstraction of the other causes of heat in working mines, such as that produced by the men and the lights.

The exact quantity of heat given out by air in proportion to its con-

densation, it is difficult to ascertain, but every day's experience proves it to be very considerable; and, I believe, this, added to the other obvious sources of heat in mines in a state of working, will be found sufficient to account for their high temperature. (Trans. Wern. Soc. vol. iv. part II. p. 466.)

IV. *On the Fusion of Charcoal, Graphite, Anthracite, and the Diamond.* By Professor Silliman.

In our fourth volume, N. S. at p. 121, we gave an account of Prof. Silliman's experiments on the fusion of charcoal; in vol. v. p. 314, some remarks on the same subject by Mr. W. West, of Leeds, were inserted; and more recently, at p. 73 of the present volume, we gave a notice respecting it by Prof. Griscom, of New York. Prof. Silliman having extended his experiments to the more difficultly combustible carbonaceous substances, has published several articles concerning them in the last number of his Journal, or that for May, the substance of which we here present to the reader.

The first article, p. 341, is a letter from Prof. S. to Dr. Hare, dated March 26, 1823, in which, after referring to his former papers, he proceeds to describe, in the following terms, the fusion of graphite by means of Dr. Hare's deflagrator.

"From a piece of very fine and beautiful plumbago, from North Carolina, I sawed small parallelopipeds, about one eighth of an inch in diameter, and from three fourths of an inch to one inch and a quarter in length; these were sharpened at one end, and one of them was employed to point one pole of the deflagrator, while the other was terminated by prepared charcoal. Plumbago being, in its natural state, a conductor, (although inferior to prepared charcoal,) a spark was readily obtained, but, in no instance, of half the energy which belongs to the instrument when in full activity, for the zinc coils were much corroded, and some of them had failed and dropped out; still the influence was readily conveyed, through the remaining coils. As my hopes of success, in the actual state of the instrument, were not very sanguine, I was the more gratified to find a decided result in the very first trial. To avoid repetitions I will generalise the results. The best were obtained, when the plumbago was connected with the copper, and prepared charcoal with the zinc pole. The spark was vivid, and globules of melted plumbago could be discerned, even in the midst of the ignition, *forming* and *formed* upon the edges of the focus of heat. In this region also, there was a bright scintillation, evidently owing to combustion, which went on where air had free access, but was prevented by the vapour of carbon, which occupied the highly luminous region of the focus, between the poles, and of the direct route between them. Just on and beyond the confines of the ignited portion of the plumbago, there was formed a belt of a reddish brown colour, a quarter of an inch or more in diameter, which appeared to be owing to the iron, remaining from the combustion of the carbon of that part of the piece, and which, being now oxidized to a maximum, assumed the usual colour of the peroxide of that metal.

"In various trials, the globules were formed very abundantly on the edge of the focus, and, in several instances, were studded around so thickly, as to resemble a string of beads, of which the largest were of

the size of the smallest shot; others were merely visible to the naked eye, and others still were microscopic. No globule ever appeared on the point of the plumbago, which had been in the focus of heat, but this point presented a hemispherical excavation, and the plumbago there had the appearance of black scoriæ or volcanic cinders. These were the general appearances at the copper pole occupied by the plumbago.

"On the zinc pole, occupied by the prepared charcoal, there were very peculiar results. This pole was, in every instance, elongated towards the copper pole, and the black matter accumulated there, presented every appearance of fusion, not into globules, but into a fibrous and striated form, like the half flowing slag, found on the upper currents of lava. It was evidently transferred, in the state of vapour, from the plumbago of the other pole, and had been formed by the carbon taken from the hemispherical cavity. It was so different from the melted charcoal, described in my former communications, that its origin from the plumbago could admit of no reasonable doubt. I am now to state other appearances which have excited in my mind a very deep interest. On the end of the prepared charcoal, and occupying frequently, an area of a quarter of an inch or more in diameter, were found numerous globules of perfectly melted matter, entirely spherical in their form, having a high vitreous lustre, and a great degree of beauty. Some of them, and generally they were those most remote from the focus, were of a jet black, like the most perfect obsidian; others were brown, yellow, and topaz coloured; others still were greyish white, like pearl stones with the translucence and lustre of porcelain; and others still, limpid like flint glass, or, in some cases, like hyalite or precious opal, but without the iridescense of the latter. Few of the globules upon the zinc pole were perfectly black, while very few of those on the copper pole were otherwise. In one instance, when I used some of the very pure English plumbago, (sawed from a cabinet specimen, and believed to be from Borrowdale,) white and transparent globules were formed on the copper side.

"When the points were held *vertically, and the plumbago uppermost*, no globules were formed on the latter, and they were unusually numerous, and almost all black on the opposite pole. When the points were exchanged, plumbago being on the zinc, and charcoal on the copper end, very few globules were formed on the plumbago, and not one on the charcoal; this last was rapidly hollowed out into a hemispherical cavity, while the plumbago was as rapidly elongated by matter accumulating at its point, and which, when examined by the microscope, proved to be a concretion in the shape of a cauliflower, of volatilized and melted charcoal, having, in a high degree, all the characteristics which I formerly described as belonging to this substance. Indeed, I found by repetitions of the experiment, that this was the best mode of obtaining fine pieces of melted charcoal.

"In some instances, I used points of plumbago on both poles, and always obtained melted globules on both; the results were, however, not so distinct as when plumbago was on the copper and charcoal on the zinc pole; but the same elongation of the zinc and hollowing of the copper pole took place as before. I detached some of the globules, and partly bedding them in a handle of wood, tried their hardness and firmness; they bore strong pressure without breaking, and

easily scratched, not only flint glass, but window glass, and even the hard green variety, which forms the aqua fortis bottles. The globules which had acquired this extraordinary hardness, were formed from plumbago which was so soft, that it was perfectly free from resistance when crushed between the thumb and finger, and covered their surfaces with a shining metallic looking coat. These globules sunk very rapidly in strong sulphuric acid—much more so than the melted charcoal, but not with much more rapidity than the plumbago itself, from which they had been formed.

"The zinc of the deflagrator is now too far gone to enable me to prosecute this research any farther at present.

"*April* 12.—Having refitted the deflagrator with new zinc coils, I have repeated the experiments related above, and have the satisfaction of stating that the results are fully confirmed and even in some respects extended. The deflagrator now acts with great energy, and in consequence I have been enabled to obtain good results when using plumbago upon *both* poles. Parallelopipeds of that substance, one-fifth of an inch in diameter and one inch or two inches long, being screwed into the vices connecting the poles, on being brought into contact, transmitted the fluid, with intense splendour, and became fully ignited for an inch on each side; on being withdrawn a little, the usual arch of flame was formed for half an inch or more. Indeed when the instrument is in an active state, the light emitted from the plumbago points, appears to be even more intense and rich than from charcoal; so that they may be used with advantage, in class experiments, where the principal object is to exhibit the brilliancy of the light.

"On examining the pieces in this, and in numerous other cases, I found them beautifully studded with numerous globules of melted plumbago. They extended from within a quarter of an inch of the point, to the distance of one-quarter or one-third of an inch all around. They were larger than before and perfectly visible to the naked eye; they exhibited all the colours before described, from perfect black, to pure white, including brown, amber, and topaz colours; among the white globules, some were perfectly limpid, and could not be distinguished by the eye from portions of diamond. In one instance only was there a globule formed *on* the point; it would seem as if the melted spheres of plumbago as soon as formed, rolled out of the current of flame, and congealed on the contiguous parts. In every instance, the plumbago on the copper side, was hollowed out, into a spherical cavity, and the corresponding piece on the zinc side, received an accumulation more or less considerable. In most instances, and in all when the deflagrator was very active, besides the globules of melted matter, a distinct tuft or projection was formed on the zinc pole, considerably resembling the melted charcoal, described in my former communications, but apparently denser and more compact; although resembling the melted charcoal, as one variety of volcanic slag resembles another, it could be easily distinguished by an eye familiarized to the appearances. In one experiment the cavity, and all the parts of the plumbago at the copper pole were completely melted on the surface, and covered with a black enamel. The appearances were somewhat varied when specimens of plumbago from different localities were used. In some instances it burnt, and even

deflagrated, being completely dissipated in brilliant scintillations; the substance was rapidly consumed and no fusion was obtained. This kind of effect occurred most distinctly when there was a plumbago piece on the copper side, and a piece of charcoal on the zinc side. I have already mentioned the curious result which is obtained when this arrangement is reversed, the charcoal on the copper, and the plumbago on the zinc side; this effect was now particularly distinct and remarkable, the charcoal on the copper side was rapidly volatilized, a deep cavity was formed, and the charcoal taken from it, was instantly accumulated upon the plumbago point, forming a most beautiful protuberance, completely distinguishable from the plumbago, and presenting when viewed by the microscope, a congeries of aggregated spheres, with every mark of perfect fusion, and with a perfect metallic lustre. I would again recommend this arrangement when the object is to attain fine pieces of melted charcoal.

" *April* 14.—In repeating the experiments to-day, I have obtained even finer results than before. The spheres of melted plumbago were in some instances so thickly arranged as to resemble shot lying side by side; in one case they completely covered the plumbago, in the part contiguous to the point on the zinc side, and were without exception white, like minute delicate concretions of mammillary chalcedony; among a great number there was not one of a dark colour except that when detached by the knife they exhibited slight shades of brown at the place where they were united with the general mass of plumbago. They appeared to me to be formed by the condensation of a white vapour which in all the experiments, where an active power was employed, I had observed to be exhaled between the poles and partly to pass from the copper to the zinc pole, and partly to rise vertically in an abundant fume like that of the oxide proceeding from the combustion of various metals. I mentioned this circumstance in the report of my first experiments, but did not then make any trial to ascertain the nature of the substance. Although its abundance rendered the idea improbable, I thought it possible that it might contain alkali derived from the charcoal. It is easily condensed by inverting a glass over the fume as it rises, when it soon renders the glass opaque with a white lining. Although there was a distinct and peculiar odour in the fume, I found that the condensed matter was tasteless, and that it did not effervesce with acids, or affect the test colours for alkalies. Besides, as it is produced apparently in greater quantity, when both poles are terminated by plumbago, it seems possible that it is white volatilized carbon, giving origin, by its condensation, in a state of greater or less purity, to the grey, white, and perhaps to the limpid globules.

" The deflagrator having been refitted only at the moment when a part of this paper had already gone to the press, and the remainder is called for, I am precluded by these circumstances from trying the decisive experiment of heating this white matter by means of the solar focus in a jar of pure oxygen gas, to ascertain whether it will produce carbonic acid gas.

" This trial I have this morning made upon the coloured globules obtained in former experiments; they were easily detached from the plumbago by the slightest touch from the point of a knife, and when collected in a white porcelain dish, they rolled about like shot, when the vessel was turned one way and another. To detach any portions

of unmelted plumbago which might adhere to them I carefully rubbed them between my thumb and finger in the palm of my hand. I then placed them upon a fragment of wedgewood ware, floated in a dish of mercury, and slid over them a small jar of very pure oxygen gas, whose entire freedom from carbonic acid had been fully secured by washing it with solution of caustic soda, and by subsequently testing it with recently prepared lime-water; the globules were now exposed to the solar focus from the lens mentioned vol. v. p. 363. It was near noon, and the sky but very slightly dimmed by vapour; although they were in the focus for nearly half an hour, they did not melt, disappear, or alter their form; it appeared however, on examining the gas, that they had given up part of their substance to the oxygen, for carbonic acid was formed which gave a decided precipitate with lime-water. Indeed when we consider that these globules had been formed in a heat vastly more intense, than that of the solar focus, we could not reasonably expect to melt them in this manner, and they are of a character so highly vitreous, that they must necessarily waste away very slowly, even when assailed by oxygen gas. In a long continued experiment, it is presumable, that they would be eventually dissipated, leaving only a residuum of iron. That they contain iron is manifest, from their being attracted by the magnet, and their colour is evidently owing to this metal. Plumbago, in its natural state, is not magnetic, but it readily becomes so by being strongly heated, although without fusion, and even the powder obtained from a black lead crucible after enduring a strong furnace heat, is magnetic. It would be interesting to know, whether the limpid globules are also magnetic, but this trial I have not yet made.

"I have already stated, that the white fume mentioned above, appears when points of charcoal are used. I have found that this matter collects in considerable quantities a little out of the focus of heat around the zinc pole, and occasionally exhibits the appearance of a frit of white enamel, or looks a little like pumice stone, only, it has the whiteness of porcelain, graduating however into light grey, and other shades, as it recedes from the intense heat. In a few instances I obtained upon the charcoal, when this substance terminated *both* poles, distinct, limpid spheres, and at other times they adhered to the frit like beads on a string. Had we not been encouraged by the remarkable facts already stated, it would appear very extravagant to ask whether this white frit and these limpid spheres could arise from carbon, volatilized in a white state even from charcoal itself, and condensed in a form analogous to the diamond. The rigorous and obvious experiments necessary to determine this question, it is not now practicable for me to make, and I must in the mean time admit the *possibility* that alkaline and earthy impurities may have contributed to the result.

"In one instance contiguous to, but a little aside from the charcoal points, I obtained isolated dark-coloured globules of melted charcoal, analogous to those of plumbago.

"The opinion which I formerly stated as to the passage of a current from the copper to the zinc pole of the deflagrator, is in my view fully confirmed. Indeed, with the protection of green glasses, my eyes are sufficiently strong to enable me to look steadily at the flame, during the whole of an experiment, and I can distinctly observe matter in different forms passing to the zinc pole, and collecting there, just

as we see dust, or other small bodies driven along by a common wind; there is also an obvious tremor produced in the copper pole, when the instrument is in vigorous action, and we can perceive an evident vibration produced, as if by the impulse of an elastic fluid striking against the opposite pole.

"If, however, the opinion which you formerly suggested to me, and which is countenanced by many facts, that the poles of the deflagrator are reversed, the copper being positive and the zinc negative, be correct, the phænomena, as it regards the course of the current, will accord, perfectly well, with the received electrical hypothesis."

We must defer the succeeding articles until our next, for want of room.

V. *Calculus of Cystic Oxide from a Dog; Constituents of that Substance, &c.*

The following are M. Lassaigne's description and analysis of a calculus extracted from the bladder of a dog, which he found in the collection of calculi belonging to M. Girard, Director of the Royal Veterinary College of Alfort.

It weighed about 38 grains troy; was of a yellowish colour, semi-transparent, of an irregular form, glossy (*lisse*) on the surface, and confusedly crystallized throughout its substance; specific gravity, 1·577. It consisted of

Cystic oxide	97·5
Phosphate and oxalate of lime	2·5

The oxalic acid could not be obtained in an uncombined state, but its existence was inferred from the property possessed by the residue of the calculus insoluble in potassa, of being partially converted into carbonate of lime by a slight calcination.

M. Lassaigne has examined the combinations of cystic oxide with potassa, and ammonia, and with the muriatic, nitric, sulphuric, phosphoric, and oxalic acids. The muriate, which is crystallized in acicular radii, consists of 5·3 acid and 94·7 oxide; the nitrate, crystallized in very slender needles, of 3·1 acid and 96·9 cystic oxide; the sulphate, a viscid uncrystallizable deliquescent substance, of 10·4 acid and 89·6 oxide, but M. Lassaigne suspects, that this compound had retained a portion of water; the oxalate, in efflorescent acicular crystals, contains 22 oxalic acid, and 78 cystic oxide.

By means of ignition with peroxide of copper, M. Lassaigne has ascertained that the composition of cystic oxide is as follows:

Carbon	36·2
Nitrogen	34·0
Oxygen	17·0
Hydrogen	12·8
	100·0

(Ann. de Chim. et de Phys. tom. xxiii. p. 328.)

VI. *Inflammation of Gunpowder by the Heat of slacking Lime.*

To determine whether the heat given out during the slacking of lime was sufficient to fire gunpowder, a small quantity of it was put into a glass tube closed at one end; the tube was then placed in slack-

ing lime and frequently removed, that it might acquire the exact temperature of the lime. Some minutes elapsed without any other effect being perceived than the volatilization of some of the sulphur of the powder, and it seemed as if no combustion would take place, but a loud explosion soon followed, without, however, breaking the tube.—(Ann. de Chim. et de Phys. tom. xxiii. p. 217.)

VII. *Cleavage of Metallic Titanium.*

Mr. W. Phillips has ascertained that the cubes of metallic titanium found in the slag of the iron-works at Merthyr Tydvil (see *Annals*, N. S. v. 67, and vi. 222), yield to mechanical division parallel to the planes of the cube.

VIII. *Formation of a Meteorological Society.*

We rejoice to state that efforts are making to establish a uniform and combined system of meteorological observation, by means of forming a Society for the purpose; the scheme has been highly approved of by several scientific gentlemen attached to Meteorology; and a meeting will be held on the third Wednesday in October, at the London Coffee House, Ludgate-hill, at eight o'clock in the evening, in order to take the subject into consideration.

Article XIV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Mr. John Shaw, the author of the Manual of Anatomy, has in the press, a work on the Distortions and Deformities to which, from various Causes, the Human Body is subject.

A concise Description of the English Lakes, and the Mountains in their Vicinity, with Remarks on the Mineralogy and Geology of the District, by Mr. Jonathan Otley.

Mr. Cottle, of Bristol, will shortly publish Observations on the Oreston Caves, and on the Animal Remains contained in them.

JUST PUBLISHED.

Meteorological Essays and Observations; by J. F. Daniell, FRS.

Description and Analysis of a New Sulphur Spring, at Harrogate; by W. West. From the Quarterly Journal of Science; with Additions by the Author.

No. 19 of G. B. Sowerby's Genera of Recent and Fossil Shells; containing the following genera:—Sigaretus, including Cryptostoma of Blainville; Stomatia, united to Stomatella; Pileolus, a new fossil Univalve, related to Nerita; Eburna, as distinguished from Buccinum spiratum and its congeners, which are usually united with it; Ranella; and Pholadomya, a new Genus of Bivalve Shells, of which a single recent Species has been lately found, but of which many fossil Species have been hitherto described as Carditæ, Lutrariæ, &c.

Article XV.

NEW PATENTS.

W. Wigston, of Derby, engineer, for certain improvements on steam-engines.—Aug. 11.

H. C. Jennings, Esq. of Devonshire-street, Marylebone, Middlesex, for an instrument or machine for preventing the improper escape of gas, and the danger and nuisance consequent thereon.—Aug. 14.

R. Rogers, of New Hampshire, in the United States of America, but now of Liverpool, Lancashire, master-mariner and ship-owner, for his improved lanyard for the shrouds and other rigging of ships and other vessels, and an apparatus for setting up the same.—Aug. 18.

J. Malam, of Wakefield, Yorkshire, engineer, for his mode of applying certain materials hitherto unused for that purpose, to the constructing of retorts and improvements in other parts of gas apparatus.——Aug. 18.

T. Leach, of Friday-street, London, merchant, but now residing at Litchfield, Staffordshire, for his improvements in certain parts of the machinery for roving, spinning, and doubling wool, cotton, silk, flax, and all other fibrous substances.—Aug. 18.

R. Higgins, of Norwich, shawl-manufacturer, for his improved method of consuming or destroying smoke.—Aug. 18.

G. Diggles, of College-street, Westminster, for his improved bit for riding horses, and in single and double harness.—Aug. 19.

E. Elwell, of Wednesbury Forge, Staffordshire, spade and edge tool maker, for certain improvements in the manufacture of spades and shovels.—Aug. 20.

M. A. Robinson, of Red Lion-street, Middlesex, grocer, for certain improvements in the mode of preparing the vegetable matter commonly called pearl barley, and grits or groats made from the corns of barley and oats, by which material when so prepared, a superior mucilaginous beverage may be produced in a few minutes.—Aug. 20.

J. Goode, of Tottenham, Middlesex, engineer, for certain improvements in machinery, tools, or apparatus, for boring the earth for the purpose of obtaining and raising water.—Aug. 20.

B. Rotch, Esq. of Furnival's Inn, London, for his improved lid for the upper masts of ships and other vessels.—Aug. 21.

J. Surrey, of Battersea, Surrey, miller, for his method of applying heat for producing steam and for various other purposes, whereby the expense of fuel will be lessened.—Sept. 4.

W. Woodman, of York Barracks, veterinary surgeon of the 2d Dragoon Guards, for his improved horse's shoe, which he denominates the beveled heeled expanding shoe.—Sept. 11.

B. Donkin, of Great Surrey-street, Surrey, engineer, for his discovery or invention on the means or process of destroying or removing the fibres from the thread, whether of flax, cotton, silk, or any other fibrous substance composing the fabrics usually termed lace net, or any other denomination of fabric, where holes or interstices are formed by such thread in any of the aforesaid fabrics.—Sept. 11.

Article XVI.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.	Daniell's hyg. at noon.
8th Mon.								
Aug. 1	S W	30·18	30·10	68	55	—	—	
2	S W	30·10	29·97	73	61	—		
3	S W	29·97	29·78	68	54	—	38	
4	W	29 90	29·78	72	52	—		
5	N W	29·90	29 90	68	52	—	—	
6	N W	29·95	29·89	69	44	—	—	
7	W	29·95	29·92	67	52	—	08	
8	W	30·01	29·90	72	47	—	04	
9	N W	30 23	30·01	65	45	·94	02	
10	S W	30·22	30·13	67	59	—	10	
11	N W	30·13	30·02	75	60	—	—	
12	S W	30 02	29·81	78	55	—		
13	N W	29 81	29·72	82	57	—	—	
14	W	29·92	29 72	65	48	—	—	
15	W	29 92	29·70	68	46	—	—	
16	S W	29·97	29·92	69	46	·84	15	
17	W	29 99	29·97	67	47	—	—	
18	S E	29·99	29·93	66	61	—	—	
19	S W	29·97	29·93	68	52	—	05	
20	S W	29·97	29·96	72	47	—	—	
21	N W	29 96	29·92	69	50	—	19	
22	W	29·92	29 80	69	49	—	10	
23	W	29·97	29·80	69	59	—	02	
24	S	29·97	29·90	71	56	—	11	
25	N W	30·07	29 97	82	55	—		
26	N E	30·20	30·07	68	57	—	55	
27	N	30·27	30·20	77	56	—		
28	W	30·27	30·12	76	52	·85		
29	S W	30·17	30·12	76	55	—		
30	N W	30·30	30·17	72	46	—	30	
31	N W	30·32	30·30	76	46	—		
		30·32	29·72	82	44	2·63	2·09	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eighth Month.—1. Overcast. 2. Cloudy. 3. Rainy. 4. Fine. 5. Showery. 6. Fine: slight showers. 7. Showery. 8. Showery: fine. 9. Showery. 10. Rainy. 11. Cloudy. 12. Fine. 13. Fine. 14, 15. Cloudy. 16. Showery. 17, 18. Cloudy. 19. Showery. 20. Fine: a slight shower at noon. 21. Cloudy. 22. Rainy. 23. Cloudy. 24. Rainy morning. 25. Fine. 26. Rainy: some distant thunder at half-past nine, a.m.: heavy rain. 27, 28, 29. Fine. 30. Fine morning: afternoon rainy. 31. Fine.

RESULTS.

Winds: N, 1; NE, 1; SE, 1; S, 1; SW, 9; W, 9; NW, 9.

Barometer: Mean height

For the month.. 29·999 inches.

For the lunar period, ending the 29th................. 29·983

For 13 days, ending the 8th (moon north)............ 29·954

For 15 days, ending the 23d (moon south)............ 29·944

Thermometer: Mean height

For the month.. 61·693°

For the lunar period.................................... 61·816

For 31 days, the sun in Leo.......................... 60·532

Evaporation... 2·63 in.

Rain... 2·09

Laboratory, Stratford, Ninth Month, 22, 1823. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1823.

Article I.

On some Anomalous Appearances occurring in the Thermoelectric Series. By the Rev. J. Cumming, MA. Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR, *Cambridge, Oct.* 13, 1823.

In forming the thermoelectric series, which you did me the favour to insert in your journal, the metallic wires were connected at one extremity, and then immersed in boiling mercury. On varying this experiment, I find some anomalous appearances which seem deserving of notice.

If one of the wires be iron, and they be heated by a spirit lamp, the deviation, in some cases, gradually attains a maximum, then returns through zero, and at a red heat assumes an opposite direction; resembling in this respect the deviations made by the alloy of antimony and bismuth mentioned in my first communication to you on this subject. These effects, the detail of which I have subjoined, took place when iron was connected with silver, copper, gold, zinc, and brass, but not with platina or lead, and I have not observed it in other cases where neither of the wires was of iron.

	Deviations.	
Iron with silver	10°	8° at red heat
——— copper	13	7 ditto
——— gold	7	4 ditto
——— brass................	17	3 ditto
——— zinc	7	3 melting zinc
	Positive	Negative

If the experiment be made by dipping wires, not previously connected, in boiling mercury, the deviation in the first instance depends, in some cases, upon the order in which they are immersed. I have observed this appearance more especially when one of the wires is copper, zinc, or brass. The results were:

Copper with gold	Copper last; negative, then slightly positive. Gold last; positive.
——— silver.....	Copper last; negative. Silver last; positive, then negative.
——— zinc	Copper last; negative. Zinc last; positive, then negative.
——— brass	Copper last; negative slightly, then positive. Brass last; positive.
——— plumbago .	Copper last; negative. Plumbago last; positive, then negative.

Copper with platina or tin was positive, with iron negative in both cases.

Zinc with silver	Zinc last; negative, then positive. Silver last; positive.
——— iron	Zinc last; negative. Iron last; positive slightly, then negative.
——— plumbago. ..	Zinc last; negative. Plumbago last; positive strongly, then negative.
——— gold	Zinc last; negative, then positive. Gold last; positive.
——— brass......	Zinc last; negative slightly, then positive. Brass last; positive.

Zinc with platina or tin was positive in both cases.

Brass with gold	Brass last; negative. Gold last; positive slightly, then negative.
——— silver	Brass last; negative. Silver last; positive slightly, then negative.
——— tin	Brass last; negative very slightly, then positive. Tin last; positive.

Zinc was positive with platina, and negative with iron, in both cases.

It is obvious that the abovementioned thermoelectric peculiarity of iron will affect its place in the series, which at a red heat will be at least above brass, though at low temperatures it is decidedly below plumbago.

In regard to the series itself, it is not, perhaps, very material, provided the order be correct, whether we consider bismuth as the most positive, and antimony as the most negative metal, or the contrary; but analogy with the galvanic series seems to make the last the preferable arrangement. In this case, which I have now adopted, antimony, heat, and bismuth, form a circuit similar to silver, acid, and zinc; the silver and antimony being the positive, and zinc and bismuth the negative poles. The corresponding thermoelectric and galvanic series will, therefore, be:

Thermoelectric Negative.	Galvanic Negative.
Galena,	Zinc,
Bismuth,	Bismuth,
Mercury,	Iron,
Nickel,	Tin,
Platina,	Lead,
Palladium,	Copper,
Cobalt,	Antimony,
Manganese,	Silver,
Tin,	Gold,
Lead,	Platina,
Brass,	Charcoal.
Rhodium,	
Gold,	
Copper,	
Ore of iridium and osmium,	
Silver,	
Zinc,	
Charcoal,	
Plumbago,	
Iron,	
Arsenic,	
Antimony.	
Positive.	Positive.

In this thermoelectric series, I have corrected an error as to the place of silver, arising from the wire formerly used, and which had been purchased as pure silver, being, as I have since found, alloyed with copper. The other changes are unimportant, excepting the insertion of galena, which in whatever way I have tried it, appears more strongly negative even than bismuth.

Believe me, my dear Sir, most truly yours,

J. Cumming.

Article II.

Remarks on the Identity of certain general Laws which have been lately observed to regulate the natural Distribution of Insects and Fungi. By W. S. Macleay, Esq. MA. FLS.*

The naturalists of the present day have in one respect a peculiar claim to the appellation of disciples of Linnæus; inasmuch as they direct their chief attention to what this great master declared to be the end of all his immortal labours in botany. His admirable maxim, that the natural system is the "*ultimus botanices finis,*" is now not only universally admitted, but on all sides acted upon. The natural system is in fact not only made the remote consequence, but the immediate aim, of every modern observation in natural history; the rule now being, to commence with supposing nothing known but what has actually been observed, and by comparing the affinities thus collected, to search after that knowledge of natural groups which in the old methods we started with supposing to be already acquired. They who formerly confined themselves to artificial systems, and neglected the above important maxim of Linnæus, have at least thereby lost much gratification, since, if there be nothing within the whole range of human science more worthy of profound meditation than the plan by which the Deity regulated the creation; so most assuredly no study is more calculated to administer pure and unmixed delight. Thus, for example, the satisfaction of the mere gazer at a collection of animals must evidently be inferior to that experienced by the comparative anatomist, who understands their respective structures. And again, the anatomist himself, on viewing a museum, can scarcely be so much gratified by the sight, as that naturalist who, not content with a bare and in some degree insulated knowledge of particular organizations, endeavours to comprehend how these harmonize with the rest of the creation. It is in this last mode alone, if I may so express myself, that the human mind can take, as far as its imperfect nature will permit, a view of the universe as it was originally designed. Nor ought any person to be deterred from commencing so delightful a pursuit, either by the supposed difficulty of the investigation, or by the extent of preparatory information which it necessarily requires; for truly has it been said, that he who questions his abilities to arrange the dissimilar parts of an extensive plan, or fears to be lost in a complicated system, may yet hope to adjust a few pages without perplexity.

Having such ideas both of the dignity of natural history and of the importance and feasibility of a more extended research

* From the Linnean Transactions for 1823, Part I.

into the natural system than has yet been made, we can scarcely fail to be interested by a late work,* of which the perusal has induced me to address this learned body. Although this work is confined to a department of botany not very generally studied, its author has evidently not been satisfied with the specific discrimination of the imperfectly organized subjects of his research, but has earnestly sought to discover the relations which they bear to each other. Keeping this object steadily in view, M. Fries has been able to give so connected and symmetrical an outline of what he considers to be the natural distribution of *Fungi*, as, at least, in my opinion, to merit the careful attention of zoologists as well as botanists. It will readily be imagined that, in saying this much, I do not, in the presence of so many more able judges, presume to advance any positive opinion on his merits as an observer. I confine myself entirely to that theory or reasoning founded by M. Fries upon the general result of observations, which it would be impossible to suppose altogether incorrect, even if his reputation as a cryptogamist were less than it really is. On this head, however, I have to remark, that our author, although undoubtedly an original observer, is neither the first who has advanced this theory, nor do *Fungi* compose the only part of organized matter in which this sort of arrangement has been conceived to exist. So that even with respect to his theory I may be a partial judge, and may probably be more inclined to admit the validity of his conclusions, than will be deemed prudent by others who are altogether unprejudiced.

M. Fries justly remarks, that the notion of the celebrated Bonnet, as to the existence of a simple series or chain of natural affinities, has been long exploded. The truth however is, that the law of continuity has been quite misunderstood both by Bonnet, and his opponents, so far as organized matter is concerned: for Bonnet fancied that, if affinities were continuous, the series must therefore be simple: and some modern naturalists finding by experience the series not to be simple, therefore supposed that affinities could not be continuous, but that nature presents to the view a mass of unconnected groups, in which it would be a waste of time and a loss of labour to search for any general plan. It does not however appear that either of these inferences has been very philosophically drawn; for there is a certain rule in natural history which originates solely in observation, and which, if properly followed up, will infallibly induce us to grant to Bonnet the truth of his proposition, that affinities are continuous, and yet to agree with his opponents, that the series of natural beings is not simple. This rule is, that *Relations of Analogy must be carefully distinguished from Rela-*

* *Systema Mycologicum* sistens Fungorum Ordines, Genera, Species, &c. quos ad Normam Methodi Naturalis determinavit, disposuit atque descripsit Elias Fries, &c. vol. i. Gryphiswaldiæ, 1821.

tions of Affinity; for, as our author M. Fries truly says, "*Quo magis in superficie acquieverunt naturæ scrutatores, eo magis analoga cum affinibus commutârunt.*"

The ideas of affinity and analogy are so distinct from each other in the mind of every person acquainted with the first principles of logic, that even while this distinction was not laid down as an axiom in natural history, experienced naturalists perceived that every correspondence of character did not necessarily constitute an affinity. Thus the celebrated Pallas, in his *Elenchus Zoophytorum,* has well observed that Bonnet, in order to complete his linear scale of nature, was obliged to abandon the true vinculum of affinity, and to resort to such superficial or analogous characters as those which connect *Vespertilio* and *Exocætus* with birds. But the nature of the difference which exists in natural history between affinity and analogy, was I believe first discovered in studying Lamellicorn Insects; and in the year 1819, when I published that discovery, the fifth part of an acute philosophical work, entitled Botanical Aphorisms,* appeared in Sweden, wherein the distinguished cryptogamist M. Agardh proves by the following words, that he likewise had a slight glimpse of the same truth: "Analogia quædam et similitudo in diversis seriebus vegetabilium interdum cernatur, quasi progressa esset natura ad perfectionem per eosdem gradus sed diversâ viâ.†"

The next work in which the distinction appeared was the *Mémoires du Muséum d'Histoire Naturelle;* in a part of which, published in the autumn of 1821, a paper was inserted by M. Decandolle on the natural family of *Cruciferæ.* Here this botanist states, that he finds it possible to express in a table all the affinities existing in this family of plants by what he terms a *double entrée;* in other words, he supposes that there are transversal affinities as well as direct ones,—a notion of the reality however which appears to be much more confused than that previously entertained by M. Agardh, and explained as above in his Botanical Aphorisms.

* *Aphorismi Botanici,* quos veniâ Ampliss. Ord. Philos. Lund. Præside Carolo Ad. Agardh, &c. pro Gradu Philosophico, p. p. N. Kuhlgren, &c. p. v. Lundæ, 1819.

† In the same little tract M. Agardh makes two other observations, which coincide with what I have noticed in the Animal kingdom. The first is as follows: "Inter inferiores formas superiores sæpe efflorescunt, sed rudes et veluti experimenta: sic anticipationes formæ perfectioris in plantis inferioribus non raro obveniant; ut etiam in plantis superioribus regressus ad formam imperfectiorem." Now in the *Horæ Entomologicæ*, p. 223, I have attempted to show that Nature, in the imperfectly constructed *Acrita*, sketches out in a manner the five principal forms of the animal kingdom. So also the direct return of Annulose *Vermes* to *Acrita* is repeatedly asserted in the same work: this however seems to depend more properly on M. Agardh's other observation, viz. "Duplex est itaque affinitas plantarum, aut ea, quæ oritur e transitu ab unâ formâ normali ad alteram, aut ea, quæ versatur imprimis in anticipatione formæ superioris aut regressu in formam inferiorem. Illam affinitatem *transitus* appellamus, hanc *transultationis*." This affinity of *transultation* is evidently nothing else than the disposition observable in opposite points of the same series or *transitus* of affinity to meet each other, and of which I have given various examples in the *Horæ Entomologicæ*, p. 319.

In the same year (1821) likewise appeared the abovementioned work of M. Fries on *Fungi*, which is explicit on the subject, and wherein the very same expressions of affinity and analogy are used to designate these different relations, which I had applied to them two years before in treating of Lamellicorn Insects.*

The theoretical difference between affinity and analogy may be thus explained:† Suppose the existence of two parallel series of animals, the corresponding points of which agree in some one or two remarkable particulars of structure. Suppose also, that the general conformation of the animals in each series passes so gradually from one species to the other, as to render any interruption of this transition almost imperceptible. We shall thus have two very different relations, which must have required an infinite degree of design before they could have been made exactly to harmonize with each other. When, therefore, two such parallel series can be shown in nature to have each their general change of form gradual, or, in other words, their relations of affinity uninterrupted by any thing known; when moreover the corresponding points in these two series agree in some one or two remarkable circumstances, there is every probability of our arrangement being correct. It is quite inconceivable that the utmost human ingenuity could make these two kinds of relation to tally with each other, had they not been so designed at the creation. A relation of analogy consists in a correspondence between certain parts of the organization of two animals which differ in their general structure. In short, the test of such a relation is barely an evident similarity in some remarkable points of formation, which at first sight give a character to the animals and distinguish them from others connected with them by affinity; whereas, the test of a relation of affinity is its forming part of a transition continued from one structure to another by nearly equal intervals. As a relation of analogy must always depend on some marked property or peculiarity of structure, and as that of affinity, which connects two groups, becomes weaker and less visible as these groups are more general, it is not in the least surprising, that what is only an analogical correspondence in one or two important particulars, should often have been mistaken for a general affinity.

M. Fries draws the distinction between them precisely in the

* I owe my acquaintance with these several works, as well as much information on points of which I should otherwise have been totally ignorant, to the friendship of the consummate botanist, in whose possession the Banksian Library has been so worthily deposited. The second part of the *Horæ Entomologicæ* was published in April, 1821. On the 24th of the following month I first saw a copy of M. Decandolle's paper, which was not published till some weeks after; and in the course of last winter I first saw Agardh's paper and the work of M. Fries on Fungi. If M. Fries borrowed from his master Agardh the idea of distinguishing affinity and analogy, which is not improbable, we must at least allow him the merit of having greatly improved this part of the theory.

† See *Horæ Entomologicæ*, p. 362 *et seq.*

same way, and, making allowance for the difference of the objects he was investigating, almost in the same words: "Natura tamen, ubique varia, semper tamen eadem, hoc est, eandem ideam exponere tendit, mutatis modo, quæ ex ulteriori ratione necessario pendent; eadem sequitur principia, ita modo ut inferiora (v. g. exterior forma, quæ in infimis adhuc vaga) superioribus cedant. Errant igitur qui distinctiones summas e formâ exteriori tantum ducunt; quis ex hac regnum animale et vegetabile definire potuit? Evidentissimè hoc demonstrant Lichenes et Fungi. Recentiores horum differentiam in characteribus externis tantum ponentes cum Fungis jungere voluerunt *Leprarias, Opegraphas, Calicia, Verrucarias, &c.* quod nullo modo probare possum. Altius illorum differentia deducenda. Sed cum natura eâdem viâ inter Lichenes et Fungos ubique progreditur, singulum genus Lichenum Fungis correspondet. At hæc inde *affinia* non dicimus; sed *analoga.*

"*Affinia* igitur sunt quæ in eadem serie sequuntur et in se invicem transire videntur. Hæc in ulterioribus congruunt sed in citerioribus rationibus differunt. *Analoga* autem dicimus quæ in diversis seriebus locis parallelis* posita sunt et sibi invicem correspondent. Ultima cosmica momenta differunt, sed citeriora congruunt, quæ in habitu externo et characteribus accidentalibus mutandis maxime valent. Ubicumque in Historiâ naturali oculos convertimus, singulum organismum multiplicia hujus offerunt exempla. Systema mycologicum infra explicatum his omnino nititur. *Clavaria* et *Peziza, Biatora* et *Bæomyces* affines sunt; sed *Clavaria* et *Bæomyces, Peziza* et *Biatora* analogæ, *e. s. p.* in infinitum.

"Comparatio Linnæana affinitatis plantarum cum mappâ geographicâ haud ignobilis visa fuit; ignoscatur igitur mihi hanc ita extendenti, ut affinitas in hac indicet longitudinem et analogia latitudinem.

"Neque hoc tantum in inferiores classes quadrat. Naturæ legis ubique harmonicæ. Si systema mycologicum et principia, quibus nititur, omnibus non displicerent, totius regni vegetabilis dispositionem demonstrare conabor. Plurima jam elaboravi."

Relations of affinity being thus separated from those of analogy, we immediately get the following facts from the observation of what M. Agardh terms the affinity of *Transitus,* namely, that species form the only absolute division in nature, and that no group of species (whatever may be the rank of these groups) ought to be considered as insulated, but only as series of affinities returning into themselves, and forming as it were circles

* As there is some danger of being led astray by our imagination when we first attempt to separate relations of analogy from those of affinity, it is fortunate that the naturalist cannot have a more admirable test of his accuracy, or a stronger rein on his fancy, than this parallelism of analogous groups in contiguous series of affinity. Thus, although a solitary resemblance may mislead, it is clear that when we find several of such resemblances to keep parallel to each other in contiguous series, we may reckon upon their having some more solid foundation than our own fancy.

which touch other circles. Such only are natural groups. This was said of Insects;* and our author, looking only at plants, and principally at *Fungi*, comes to the same conclusion, as appears from the following words: "Species unica in naturâ fixè circumscripta idea. Superiores nullas agnovimus sectiones strictissimè circumscriptas, tantum circulos plus minus clausos, affines vero ubique tangentes. Hos tribus, genera, sectiones, &c. simulque si naturæ vestigia sequuntur, naturales dicimus."

That the circle, indeed, is not always closed or complete has been observed likewise in the animal kingdom; and there are two ways of accounting for it. First, that the beings which would render the circle complete have not yet been discovered; a conclusion to which we readily arrive on considering how little is yet known of natural productions; and secondly, that there are *hiatus* or chasms which do really exist in nature, and which may be attributed to the extinction of species in consequence of revolutions undergone by the surface of this globe. Whether one only or both of these reasons be requisite to account for circles of affinity not always appearing complete, we shall not at present investigate; contenting ourselves with the undoubted fact, that *hiatus* or chasms are everywhere in nature presenting themselves to the view. But this truth by no means contradicts the Linnean maxim, that no *saltus* exists in nature, although such has been esteemed its effect by certain naturalists who have been in the habit of taking the words *hiatus* and *saltus* as synonymous terms.† Thus the series of the *Systema Naturæ* and of the *Règne Animal* is not natural where the *Cetacea* intervene between Quadrupeds and Birds, but is perfectly consonant with nature where the Tortoises are made to follow these last. In the first case, there is a *saltus* or leap from Quadrupeds to Birds over a group totally dissimilar to the latter; there is, in short, an unnatural interruption of the law of continuity, which shocks not merely the naturalist but the ordinary observer. In the other case there is only an *hiatus* or chasm, which the discoveries of a future day may fully occupy. Speaking therefore theoretically, it may be affirmed that a *saltus* never did exist in nature; and it also may be argued, with great appearance of truth, that if the *hiatus* are real which so commonly occur in nature, they did not always exist; or, in short, as M. Fries expresses himself, "*Omnis sectio naturalis circulum per se clausum exhibet.*"

Now this definition of a natural group could never have been given by any person who was not aware of the distinction to be made between affinity and analogy. But whenever two parallel

* *Horæ Entomologicæ*, p. 459, &c.

† It is to be regretted that Prof. Dugald Stewart should have been led into this common error, and thus have acquired a somewhat erroneous notion of the law of continuity, as it refers to natural history. See the second part of his admirable Dissertation, as prefixed to vol. v. of the *Supplement* to the *Encyclopediæ Britannica.*

series of objects linked by affinity are drawn up in array, the connexion of their extremes, that is, the formation of the circle, becomes in that very moment, so far as I have observed, more or less conspicuous.

It follows, moreover, from admitting the existence of analogical relations, or, in other words, from laying down the parallelism of groups in different series of affinity, that the number of groups in these series must be the same. For were it otherwise, as for instance, supposing three groups to exist in one complete series, and four in another, it is clear that the parallelism could not exist. But if this parallelism be real, which has been, as shown above, asserted independently of each other by several naturalists acting in different branches of natural history, then the number of groups of the next lower order composing a group of a given degree must be determinate. And if, moreover, we accord to our author the accuracy of the following rule, namely, " Nunquam negligendum, unumquodque regnum, ordinem, genus, &c. in systemate ut individuum esse sumendum;"—in other words, that class bears the same relation to class which order does to order, and genus to genus; then the number of groups composing *any* group of the next higher degree must be determinate; and it only remains for the naturalist to discover from observation what this number is.

That Nature has made use of determinate numbers in the construction of vegetables has long been known empirically; as for instance, where botanists have found the typical number of parts of fructification in the acotyledonous plants of Jussieu to be two, that in monocotyledonous plants to be three, and that in dicotyledonous plants to be five, or multiples of these numbers. Consequently the existence of a determinate number in the distribution of the plants themselves might have been argued *à priori*. And in this manner indeed M. Fries appears to have argued; for it is tolerably clear that it was the consideration of the foregoing rule, adopted by Nature in the structure of acotyledonous plants, which induced him theoretically to assume four as a multiple of two to be the determinate number in which *Fungi* are grouped.* I say this, because he is obliged from actual observation to admit that of these four groups, one is excessively capacious in comparison with the other three, and is always to be divided into *two*. So that we may either, with M. Fries, consider every group of *Fungi* as divisible into four, of which the largest is to be reckoned as two,—a supposition that would not only make two determinate numbers, but which, from the binary groups not being alway analogous, will moreover

* It ought here to be observed, that Ocken had previously advanced the opinion that four was the determinate number in natural distribution. This naturalist, however, having in his *Naturgeschichte für schulen*, lately published, in a great measure abandoned the number four for five, and that more especially in the animal kingdom, has thus got into all the difficulties which necessarily attend the supposition of two determinate numbers.

break the parallelism of corresponding groups,—or we may account every group as divisible into five, and thus not only agree with M. Fries's observations, but besides keep the parallelism of analogies uninterrupted. If in this state of the matter it could now be shown, that in the animal kingdom the same law is followed by nature; in short, to take an instance, if it could be proved that the *Annulosa* may either be divided into four groups, viz. *Ametabola, Crustacea, Arachnida* and *Ptilota,* where this last is remarkably capacious and divisible into two natural groups, viz. *Mandibulata* and *Haustellata,* or that annulose animals may be divided at once into five groups of the same degree, but of which two have a greater affinity to each other than they have to the other three—if, I repeat, this could be proved, should we not be justified in affirming that the rule, so far as concerns Insects and Fungi, is one and the same? The possibility of thus distributing the annulose animals has, however, been demonstrated already in the *Horæ Entomologicæ;* and it is the way in which we ought to take the rule that only now remains to be investigated. In short, since only two methods * have yet been found to coincide with facts as presented by nature, the question is, whether we ought to account *Fungi* as divisible into five groups, or into four, of which one forms two of equal degree. Now I think it may without difficulty be shown, from our author's own observations and rules, that there is only one determinate number which regulates the distribution of *Fungi,* and that five is this number.

In the first place, M. Fries lays it down as a rule, which is quoted above, that he admits no groups whatever to be natural unless they form circles more or less complete. Let us then apply this rule to what he terms his central group, and which he makes always to consist of two. Does this form a circle? If not, the group cannot be natural according to his own definition.

If, on the other hand, its two component groups are each circles, then these are natural. Thus the *Ptilota* will not form one circle, but two; consequently they form two natural groups, which is furthermore proved by their parallel relations of analogy. If we turn to Fungi also, the *Hymenini,* according to

* The number seven might also perhaps, for obvious reasons, occur to the mind, were it allowable in natural history to ground any reasoning except upon facts of organization. The idea of this number is however immediately laid aside, on endeavouring to discover seven primary divisions of equal degree in the animal kingdom. It is easy, indeed, to imagine the prevalence of a number; the difficulty is to prove it. The naturalist, therefore, requires something more than the statement of a number, before he allows either a preconceived opinion or any analogy not founded on organic structure to have an influence on his favourite science. He requires its application to nature and its illustration by facts. As yet, however, no numbers have been shown to prevail in natural groups but five, or, which is the same thing, four of which one group is divisible into two. Perhaps, indeed, the most clear method of expressing ourselves on this subject is to say that, laying aside osculant groups, every natural group is divisible into five, which always admit of a binary distribution, that is, into two and three.

M. Fries, do not form one circle, but two; one of *Pileati*, the other of *Clavati*; so that instead of the *Hymenomycetes* forming four natural groups, viz. *Sclerotiacei*, *Tremellini*, *Uterini*, and *Hymenini*, they form, if our author be correct, five; viz. *Sclerotiacei*, *Tremellini*,* *Uterini*, *Pileati*, and *Clavati*.

But to understand this still better, we had as well perhaps enter a little deeper into our author's theory. Every group, he says, which expresses well the character of the superior group to which it belongs, is called the *centrum*; by this, not meaning the centre of a circle, but the site of the normal form or perfection of the particular structure common to the superior group, of which it forms a part. The word *perfection*, even as here used, requires explanation; for it does not, as might be supposed, in this place signify affinity to any particular group. Our author, on the contrary, most properly says, that the idea of perfection in structure has nothing to do with affinity.† "Ipsa hæc affinitas imperfectionem potius indicat; perfectissima enim sunt in quâvis sectione ab omnibus aliis remotissima. Sic perfectissima animalia et vegetabilia, quæ maxime a se invicem remota; infima, quorum limites confluunt." Hence it follows, that the *centrum*, or perfection of a group, is in fact that part of the circumference of the circle of affinity which is farthest from the neighbouring group, and exactly the same thing with what in the *Horæ Entomologicæ* has perhaps more happily been called *Type*.

Indeed the confusion arising from the use of the word *centrum*, as applied to a point in the circumference of a circle, is still increased by applying the word *radii* to those groups likewise in the circumference which lead from one centrum or type to another, and which I have termed *annectent groups*.‡ The use of these terms *centrum* and *radii* is the more unfortunate, as our author never for a moment takes them in any other sense than that in which I have used the expressions *type* and *annectent groups*. When, therefore, he says that in every group, whether class, order, &c. there are a *centrum* and *radii*, we must under-

* This appears to be one of those interesting groups which connect the least perfectly organized beings with those which are the most perfectly organized. In the department of *Hysterophyta* it is to the *Coniomycetes* or lowest *Fungi*, what in the animal kingdom the *Vermes* are to the *Acrita*.

† To the general observations on this subject, as connected with the animal kingdom, which I have given in *Horæ Entomologicæ*, p. 205, I may add the botanical authority of Prof. Schweigger. "Nec etiam genera et ordines plantarum in lineam a cryptogamicis ad dicotyledoneas progredientem ita disponi possunt, ut familia quævis præcedentis structuram magis evolutam præbeat. Vix ullus de vegetabilium serie usitata, a cotyledonum numero deducta, affirmat, plantas dicotyledoneas omni ratione monocotyledoneis esse anteponendas." p. 6. *De Plantarum classificatione naturali Disquisitionibus Anatomicis et Physiologicis stabilienda Commentatio, Auctore A. F. Schweigger, &c. Regiomonti* 1820.

‡ There are several other terms used by M. Fries to designate his groups, and which differ from those employed by me to express the nature of similar groups. Thus, his *intermediate genera* are my *osculant genera*; his *subordinate genera* are my *types of form* or *sub-genera*, &c.

stand him as meaning, that there are in every circle first a *type* or normal form expressing the perfection of the superior group to which it belongs; and secondly, *annectent groups* connecting this type with other groups. Or, to take his own words, "In *centrum* quod species plurimas continet, character optime quadrat. *Radii* ad reliquas classes (scilicet ordines, genera, &c.) abeuntes, utriusque classis characterem conciliant, sed ad illam (viz. the typical group) cujus character maxime eminet referuntur."

If then the determinate number in which *Fungi* are naturally grouped be four, and if it thus appears that, according to M. Fries, every natural group is a circle, having in its circumference a point of perfection or typical group called a *centrum*, and annectent groups called *radii*, it is evident that there must be one centrum and three radii for every group. But observe what immediately follows as the result of M. Fries's observation: "*Centrum* abit semper in duas series, inferiorem et superiorem, quarum illa ad antecedentem hæc ad sequentem classem (l. radium) evidentius accedit."

This rule being determined, M. Fries goes on moreover to say, that these two series which compose the *centrum* are always analogous at their corresponding points. Consequently, in every circle he admits the existence of two central groups and three radial; that is, in all, five natural groups. Now this truly is the case throughout the whole animal kingdom. Organized matter is the *centrum* of matter, and is composed of animals and vegetables. *Articulata*,* or animals possessing an articulated axis, form the centrum of the animal kingdom, and are composed of *Vertebrata* and *Annulosa*. The *Ptilota* of Aristotle, or winged insects, form the centrum of the *Annulosa*, and are divided into *Mandibulata* and *Haustellata*. And so on, we shall ever find a natural group to be a circle of five minor groups, and that two of these minor groups form what M. Fries would call a *centrum*, or, more correctly, have some character in common which distinguishes them from the other three. That neither of these groups, viz. organized matter, *Articulala* or *Ptilota*, is a circle, must be obvious to every observer: and consequently they do not fall within the sphere of M. Fries's definition already given of a natural group, but each of them form two circles, which therefore, according to our author, are natural groups. We might turn even to the well-known great division of the vegetable kingdom into phænogamous or cotyledonous and cryptogamous or acotyledonous plants, where the former are clearly the *centrum*, and divisible into two natural groups; but surely enough has been said to show, that the notion of M. Fries on this head is in every respect, but the mode of expressing it, the same identically with mine. When he states the determinate

* This name has been applied to the *Annulosa*, as characterizing them alone, but improperly, inasmuch as the vertebrated animals are articulated.

number to be four, and we investigate the signification attached by him to this proposition, we discover that it is in effect five. How M. Fries was led to the number four, we have already endeavoured to èxplain; and it is truly worthy of observation, as an almost conclusive argument for the determinate number being five, that M. Fries himself is at last obliged to adopt it. This open abandonment of his theoretical number *four*, which we have seen that he had virtually abandoned before, takes place moreover in that part of his work which, relating to the more minute groups, is therefore most independent of theory, and most subjected to the keenness of practical observers. Here, in brief, he finds himself tied down to stubborn facts, and it is rather interesting to mark the result. The only genera of *Hymenomycetes Pileati* which he discovers to be divisible are, *Agaricus, Cantharellus, Thelephora, Hydnum, Boletus, Polyporus,* and *Dædalea;* some of which, as *Agaricus,* are, as he says, of the first dignity; others, as *Cantharellus,* of the second.* Now every one of these genera, or at least their typical groups, are divided by M. Fries himself into five, with the single exception of *Cantharellus;* and so truly natural or dependent upon relations of analogy are these five subdivisions, that he proposes to make use of one set of names for all, and in fact does in general make use of the same name for analogous groups.† Nay more: when he has divided the well-known genus *Agaricus* into *five* natural series, he observes, "Singula series a naturâ fixè determinata clausa est reliquis parallela. Tribus diversarum serierum analogas diu eodem nomine salutavi. So that *Agaricus* is, according to the confession of M. Fries, formed of five natural series each closed up; in other words, each a circle, and corresponding at their parallel points to such a degree, that he declares it possible to assign the same names to the analogous groups.

It were tedious to proceed much further on this subject; and therefore, without entering into the speculations, often unintelligible and always vague, of Plutarch, Sir Thomas Brown, Drebel, Linnæus and others, as to the doctrine of *quintessence* generally, we may at once set forth the last argument which shall now be produced for the existence of a quinary distribution in organized nature. It may be stated thus: In the year 1817 I detected a quinary arrangement ‡ in considering a small portion of coleopterous insects; and in the year 1821 I attempted to show that it prevailed generally throughout nature. In the same year (1821), and apparently without any view beyond the particular case then before him, M. Decandolle stated the natural distribution of Cruciferous plants to be quinary. And again,

* The groups here said to be of the second dignity, appear to be of the same degree with the general *Phanæus* and *Scarabæus* of the *Horæ Entomologicæ.*

† These five names are, *Mesopus, Pleuropus, Merisma, Apus,* and *Resupinatus.*

‡ Published in 1819.

in the same year, a third naturalist, without the knowledge of either Decandolle's *Mémoire* or the *Horæ Entomologicæ*, and in a different part of Europe, publishes what he considers to be the natural arrangement of *Fungi.* Arguing *à priori*, this third naturalist fancies that the determinate number into which these acotyledonous plants are distributed ought to be four; but finds it necessary, in order that it may coincide with observed facts, to make it virtually five. Nay, at last, in spite of the prejudice of theory, he is unable to withstand the force of truth, throws himself into the arms of Nature, and declares that where he actually finds his natural group complete in all its parts, there the determinate number is *five*.

Now, on considering that his work was given to the world two years after the first part of the *Horæ Entomologicæ*, it is clear that, had M. Fries fixed at once on the number five, there might have been room for supposing, that he had not altogether trusted to his own observation, but had borrowed the idea of a quinary distribution. As matters however at present stand, this supposition cannot for a moment be harboured; and I cannot help rejoicing that the strength of this beautiful theory should be so completely brought home to the conviction of every mind, as it must be, by observing the manner in which different persons have respectively stumbled upon it in totally distinct departments of the creation. We may all possibly be wrong in part, or even in much of our respective details; but however this may be, it is difficult not to believe that we are grasping at some great truth, which a short lapse of time will perhaps develop in all its beauty, and at length place in the possession of every observer of nature.

It may be well to note, that M. Fries draws in the clearest manner a distinction between his *Hysterophyta* or *Fungi*, and the *Protophyta*, which is a natural group consisting of the Linnæan *Algæ* and *Lichenes.* He proves that they form two distinct series of vegetables having analogous exterior forms at their corresponding points. Hence, according to what has preceded, the *Protophyta* and *Fungi* form in the vegetable kingdom two primary groups of equal degree. In *Protophyta* fructification is secondary, and the thallus essential; whereas in *Fungi* it is quite the reverse. According to our author the first-born of Flora may all be accounted as essentially *roots*, and representing the mode of nutrition; while every fungus is as truly and representatively connected with fructification and reproduction. Throwing aside other considerations, we may perceive the analogous groups of the animal kingdom to be likewise constructed on a similar plan. Each of the *Acrita*, for example, imbibing nourishment at every pore of their surface, internal or external, is essentially a stomach, while the situation of the singular ovaries of the *Radiata* cannot fail to remind us of the importance and position of the sporidia in *Fungi.* The umbellate *Medusa*,

the *Echinus,* the *Asterias,* and the *Priapulus* have all their representatives in mycology, of which the genera *Lycoperdon* and *Phallus* are noted instances; so that the analogy of the Radiated animals to *Fungi* is complete; and we thus have in organized matter the following two series of groups connected by affinity and analogous at their corresponding points.

ANIMALIA.	VEGETABILIA.
Acrita	Protophyta.
Radiata.	Hysterophyta.
Annulosa	Monocotyledonea.
Vertebrata.	Dicotyledonea.
Mollusca	Pseudo-cotyledonea? *Agardh.**

Consequently some general idea of the primary distribution of all organized beings may be obtained from the following figure.

* This last department of the vegetable kingdom, *Pseudo-cotyledonea*, has been defined by M. Agardh in the sixth part of his *Aphorismi Botanici*, which is dated Dec. 1821. According to him it embraces the *Musci*, *Hepaticæ* and *Filices* of Linnæus; and in p. 76 of the same work we find a comparison made between these plants and *Amphibia*, which is nevertheless much stronger when applied to them and the *Mollusca*. "Pseudo-cotyledoneæ Amphibiis non dissimiles, humum perreptant vel rimas quærunt, humiditateque gaudent ut illa, organis jam in superiore sectione deperditis iterum instructæ." In these last words he alludes to his own opinion, that Mosses display organs nearly related to the cotyledons of dicotyledonous plants, while the monocotyledonous plants conceal their cotyledon; and if botanists should adopt this opinion, we might assimilate it to the curious fact, that in the animal kingdom the imperfectly organized *Mollusca* display a heart, which is more analogous to that of the *Vertebrata* than the dorsal vessel of insects. With respect, indeed, to the analogies existing between the animal and vegetable kingdoms, they are too striking to have altogether escaped the notice of such an observer as Agardh, who truly observes, "Memorabilis est analogia evolutionis seriei vegetabilis cum animali." When we find him, however, comparing the least perfect vegetables to some of the most perfect animals, the *Algæ* to Fishes, and the *Lichenes* to Insects, we must suspect that he is not sufficiently acquainted with the *evolution* of the animal series, and conclude that he has at least not sufficiently attended to the parallelism of analogy. Nevertheless, his comparison of Monocotyledonous, or, as he terms them, of Cryptocotyledonous Plants to Birds, appears to be a true relation of analogy, although an indirect one; and if he had paid that attention to Entomology which the science really merits, so acute a botanist, could not have failed to perceive, that the arguments he gives in support of this last analogy, only receive their full force when they are employed in the comparison of Monocotyledonous Plants with Insects. Thus, in the same page, he states aëriferous cells to be peculiar to Birds in the animal kingdom, evidently not aware that many more animals than are in the whole department of *Vertebrata* would have no means of getting their fluids aërated did not the air enter their bodies and penetrate through every part of them. But on this head Desfontaines long since set the scientific world at rest, when he established the relation of Dicotyledonous Plants to *Vertebrata*, and of Monocotyledonous Plants to *Annulosa*, not on external appearance merely, but on such primary principles of their respective structures, that we may almost term the former tribe of plants Vertebrated, and the latter Annulose. It would scarcely be fair however towards M. Agardh, did we conceal the fact of his being perfectly aware of the analogies which reign both between the Dicotyledonous Plants and the typical group of *Vertebrata*, and between the *Fungi* and *Radiata*. With respect to this last analogy, indeed the following words are perhaps more explicit than those previously published, p. 211 of the *Horæ Entomologicæ*—"Fungi superiores animalia Radiata ob figuram radiantem, ob superficiem nudam, ob texturam laxam, ob colorem subsimilem non male revocant."

To conclude: If an arrangement be natural, it will stand any test; and to support the truth of this proposition, I shall now arrange Annulose Animals in the same way that M. Fries has distributed his Fungi, when it will readily be seen as virtually nothing else than the arrangement I offered to the public in the *Horæ Entomologicæ*. Thus it is only necessary that instead of subjecting Nature to arbitrary rules of our own invention, we should humbly receive her laws as she clearly proclaims them; when she will indeed appear, as M. Fries has found her to be, "ubique varia, semper tamen eadem."

Classification of ANNULOSA *on the same Principles as those adopted by M. Fries in his natural Distribution of Fungi.*

ANNULOSE ANIMALS, which are not hermaphrodite: or the ANNULOSA of Scaliger may all be divided into two groups founded on their larva or fœtus state, viz.

1. *Apterous Insects*, having either no metamorphosis in the usual sense of the word, or only that kind of it the tendency of which is confined to an increase in the number of feet.

These are the APTERA of Linnæus, and comprehends three classes, viz. *Crustacea*, *Arachnida*, and *Ametabola*, which would be termed *Radii* by M. Fries.

2. *True Insects*, being all subject to that kind of metamorphosis which has a tendency to give wings to the perfect or imago state, but never more than six feet.

These are the PTILOTA of Aristotle, and should, according to M. Fries, be termed the *Centrum* of Annulose Animals. "*Sed centrum abit semper in duas series,*" and consequently we find that the

PTILOTA

either become by metamorphosis organized for mastication in their perfect state, and are the	or become by metamorphosis organized for suction in their perfect state, and are the
MANDIBULATA of *Clairville*, which comprise the following orders, viz.	HAUSTELLATA of *Clairville*, which comprise the following orders, viz.
1. Metamorphosis obtect. Larvæ eruciform. TRICHOPTERA?	1. Metamorphosis obtect. Larvæ eruciform. LEPIDOPTERA.
2. Metamorphosis incomplete, or coarctate. Larvæ apod or vermiform. HYMENOPTERA.	2. Metamorphosis incomplete, or coarctate. Larvæ apod or vermiform. DIPTERA.
3. Metamorphosis incomplete. Larvæ of various types. COLEOPTERA.	3. Metamorphosis incomplete. Larvæ .... APTERA. *The only larva of this order known is apod or vermiform, but of the coleopterous structure.*
4. Metamorphosis semicomplete. Larvæ resembling the perfect Insects. ORTHOPTERA.	4. Metamorphosis semicomplete. Larvæ resembling the perfect Insects. HEMIPTERA.
5. Metamorphosis various. Larvæ hexapod. NEUROPTERA.	5. Metamorphosis various. Larvæ hexapod. HOMOPTERA.

N. B. A mark of doubt is annexed to the word *Trichoptea*, because entomologists have not yet determined whether the Linnæan genus *Phryganea* forms part of an annectent order, or whether it forms a distinct osculant order.

Article III.

On the Composition and Equivalent Numbers of certain crystallized Muriates. By R. Phillips, FRS. L. and E. &c.

While correct views of the nature of chlorine and most of its compounds have been derived from the researches of Sir H. Davy, it appears to me that the attention of chemists has not been sufficiently directed to the consideration of the nature of some compounds which may be considered either as muriates or as chlorides containing water. In the first volume of the *Annals,* N. S. I gave a statement of the different views which may be entertained of those salts, which must be regarded either as chlorides or muriates. I now return to the subject, from having lately had occasion to employ the salt usually called muriate of barytes in such proportions as contained a certain quantity of the earth.

In order to ascertain the equivalent number of this salt, I consulted Dr. Thomson's table of the weights of atoms, given in the last volume of his System of Chemistry; in this we find the composition of chloride of barium, but not of muriate of barytes; and under the head of muriate of barytes (vol. ii. p. 254), the reader is referred to chloride of barium for a description of it. "The easiest method of preparing it," says Dr. Thomson, "would be to dissolve carbonate of barytes in muriatic acid, and crystallizing the solution." "The primitive form of this chloride," he continues, "is, according to Haüy, a four-sided prism, whose bases are squares. It crystallizes most commonly in tables." (System, vol. i. p. 357.) From this quotation it is, I think, evident, that Dr. Thomson considers the crystallized muriate (for so at present I shall continue to call it) as a mere chloride, and he does not mention that it contains any combined water: he certainly observes, "that when heated, it decrepitates and dries," but this seems merely to refer to accidental moisture.

On Dr. Wollaston's scale, dry muriate of barytes is mentioned, and this is of course the chloride of barium, for the number by which it is represented agrees as nearly with that assigned by Dr. Thomson to the chloride as 131 to 132·5 In the memoir in which the scale is described by its author, crystallized muriate of barytes is represented as consisting of muriate of barytes 131 + 2 water = 22·6; making the number for crystallized muriate of barytes 153·6; it is singular that Dr. Wollaston has not placed this upon the scale, for as the salt usually occurs in the crystallized state, it is that in which it is most used, and in which the knowledge of its equivalent is most desirable. Mr. Brande (Manual, vol. ii. p. 82), appears to agree with Dr. Thom-

son in considering the crystallized salt as mere chloride of barium. This, he observes, "may be obtained by heating baryta in chlorine, in which case oxygen is evolved; or more easily by dissolving carbonate of baryta in diluted muriatic acid. By evaporation, tabular crystals are obtained, soluble in five parts of water at 60°; and consisting when dry of 65 barium + 33·5 chlorine = 98·5. Its taste is pungent and acrid; when exposed to heat, the water of crystallization separates, and the dry chloride enters into fusion." It is to be observed, that while Mr. Brande admits the existence of water of crystallization in this salt, he neither states its quantity, nor makes any observation as to the effect which it may produce in the theoretic views of the nature of the salt. On referring to the table of equivalents contained in the second volume of the Manual, p. 512, and to that which Mr. Brande has since published in the 14th volume of the Royal Institution Journal, I do not find any mention of muriate of barytes, or of the quantity of water which the crystallized chloride, allowing it to be such, contains.

Dr. Ure, in the second edition of his Dictionary, mentions the muriate of barytes as crystallized in tables; and although he calls it a muriate, he states its composition as a chloride, consisting of 4·5 chlorine + 8·75 barium.

No mention of muriate of barytes is made by Dr. Henry, in his Elements of Chemistry, excepting under the head of chloride of barium; and like the previously quoted authorities, he appears to consider the crystallized tabular salt as chloride; but does not mention the existence of any water in it. "The dry salt," he observes, "Sir H. Davy considers as a compound of 1 atom of barium = 70 + 1 atom of chlorine = 36; hence its representative number is 106, and it consists of

Chlorine	34
Barium	66
	100

"Muriate of baryta, formed by the action of water on the chloride, must therefore be constituted of 1 atom of muriatic acid = 37, + 1 atom of baryta = 78, and its equivalent must be 115. Hence it should consist, when crystallized, of

Acid	27·82	= 1 atom
Baryta	58·47	= 1 atom
Water	13·71	= 2 atoms
	100·00	

"These numbers do not exactly agree with the experimental results of Aikin and Berzelius, which state its composition as follows;

	Acid.	Base.	Water.
According to Mr. Aikin.	22·93	62·47	14·6
———— Berzelius	23·35	61·85	14·8

"The analysis, therefore, requires to be attentively repeated."

Now I would submit, with great deference, that the analyses which have been already performed are sufficient to clear up the difficulty, which, it appears to me, depends merely upon the mode of viewing the nature of the salt in question.

The various metallic chlorides, and the different salts which result from the union of muriatic acid with metallic oxides, may be regarded under several different theoretical points of view: these I shall endeavour to illustrate by considering the barytic chloride and muriate. According to Dr. Thomson's table of equivalents, oxygen being represented by 8, water is 9, chlorine 36, muriatic acid 37, barium 70, and barytes 78; and if we admit for a moment, the existence of what was formerly called dry muriatic acid, its number will be 28.

Chloride of barium is then composed of

One atom of chlorine.	36
One atom of barium	70
	106

Supposing, as appears to be the case with muriate of magnesia, that a solution of barytes in muriatic acid could be evaporated to dryness without the formation of water occurring from the decomposition of the acid and oxide, and the union of their hydrogen and oxygen, we should then procure muriate of barytes composed of

One atom of muriatic acid.	37
One atom of barytes	78
	115

This compound would also result from the decomposition of one atom of water by an atom of chloride of barium.

Considering, according to the opinion formerly adopted by most chemists, and still entertained by Berzelius, that muriatic acid gas is a compound of dry muriatic acid and water, dry muriate of barytes will consist of

One atom of dry muriatic acid.	28
One atom of barytes	78
	106

This number, it will be observed, is that which has already been noticed as representing the chloride.

It is a question which, perhaps, scarcely admits of being decided, whether when an oxide, such as that of barium, calcium, or strontium, is dissolved in muriatic acid, and a crystalline salt containing water obtained, such salt be actually a chloride combined with water, or whether one atom of the water suffers decomposition and converts the chloride into a muriate. Supposing, for example, that 115 parts of crystallized muriate of barytes were to lose 9 of water by being heated, that salt before such loss might be regarded as hydrous chloride of barium consisting of

1 atom of chlorine	36
1 atom of barium	70
1 atom of water	9
	115

Or we may consider the 9 parts of water expelled by heat not as previously existing as such, but as arising from the decomposition of the muriatic acid and oxide of barium; in which case the salt would be composed of

1 atom of muriatic acid	37
1 atom of barytes	78
	115

We find, however, by experiment, that crystallized muriate of barytes loses a larger quantity of water than that above supposed. According to Mr. Aikin (Nicholson's Journal, vol. xxii. p. 312), crystallized muriate of barytes loses from 14·5 to 14·6 per cent. of water, by being heated, a determination which agrees very nearly with Berzelius's statement of 14·8 per cent. If an atom of chloride of barium = 106 were combined in the crystallized salt with 2 atoms of water = 18, then the loss by heat would a little exceed 14·51 per cent. agreeing very nearly with Mr. Aikin's statement. We may, therefore, consider crystallized muriate of barytes as consisting of

1 atom of chloride of barium	106
2 atoms of water	18
Weight of its atom	124

Or,

Chlorine	29·03
Barium	56·45
Water	14·52
	100·00

Or if we consider the salt to exist in the state of muriate, the view of its composition will be:

1 atom of muriate of barytes	115
1 atom of water	9
	124

Or,

Muriatic acid	29·84
Barytes	62·90
Water	7·26
	100·00

Or lastly, if we adopt the opinion formerly entertained of this salt, and consider it as composed of

1 atom of dry muriate of barytes.	106 or	85·49
2 atoms of water.	18	14·51
The weight of its atom will still be.	124	100·00

agreeing very nearly with Aikin, Berzelius, and Dr. Wollaston's memoir.

There are but few salts similarly circumstanced with the crystallized muriate of barytes; I shall add the equivalent numbers for crystallized muriate of strontia and muriate of lime.

According to Berzelius (Proportions Chimiques, p. 47), muriate of strontia contains 40·53 per cent. of water; the chloride of strontium will therefore be 59·47. Now an atom of chlorine = 36, and of strontium = 44, will give 80 as the weight of the atom of chloride of strontium, and as 59·47 : 40·53 :: 80 : 54·52, so little exceeding 54, or 6 atoms of water, that we may consider crystallized muriate of strontia as composed of

1 atom of chloride of strontium 36 + 44 =	80 or	59·7
6 atoms of water 9 × 6 .... =	54	40·3
Weight of atom	134	100·0

Or regarding it as a crystallized muriate of strontia, it will consist of

1 atom of muriate of strontia 37 + 52 =	89 or	66·4
5 atoms of water 9 × 5 .... =	45	33·6
	134	100·0

Crystallized muriate of lime, according to Berzelius, contains 49·2 per cent. of water; the chloride of calcium remaining will, therefore, amount to 50·8. An atom of chlorine = 36, and of calcium = 20, the number representing chloride of calcium is 56; and as 50·8 : 49·2 :: 56 : 54·23, so slightly exceeding 54, that we may regard crystallized muriate of lime as constituted of

1 atom of chloride of calcium 36 + 20	=	56 or	50·9
6 atoms of water 9 × 6.	=	54	49·1
Weight of atom		110	100·0

Or,

1 atom of muriate of lime 37 + 28. ..	=	65 or	59·09
5 atoms of water 9 × 5	=	45	40·91
		110	100·00

Article IV.

On the Deluge. By J. S. Henslow, MA. MGS. FLS. Secretary to the Cambridge Philosophical Society, Professor of Mineralogy in the University of Cambridge.

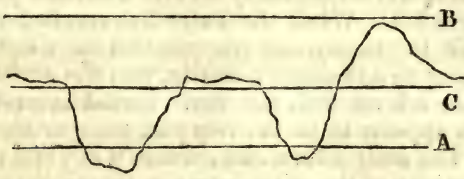
(To the Editor of the *Annals of Philosophy*.)

SIR, *Cambridge, Oct.* 15, 1823.

In a very able article in the 57th number of the Quarterly Review, the "Reliquiæ Diluvianæ," by Prof. Buckland, has been lately examined, and towards the end of that article some observations are made upon the various theories which have been adopted to account for the phenomena of the Deluge. The reviewer is decidedly of opinion that none of the hypotheses hitherto suggested are capable of solving the difficulty, and seems to think that we ought to ascribe the whole to the miraculous interposition of Providence, "*excluding* the operation of ordinary nature" from our consideration. That God brought the waters, and that God caused them to assuage, is doubtless the language of Scripture; but, as in many other cases, so in the present, I see no reason for supposing that he did not employ the ordinary means of nature as the instruments of his operations. The reviewer himself states his belief that "miraculous agency is often, nay generally, combined with natural means," though he seems at the same time anxious to dispense with them in the present case.

The hypothesis which had hitherto appeared the most plausible was one stated by Mr. Greenough, in which it is supposed that the waters of the ocean were thrown into a state of excessive agitation by the near approach of a comet. This hypothesis however, is now clearly shown to be incompatible with the appearances observable in the diluvium of various parts of the earth; and it should also be recollected that the near approach of a comet could not have produced the effect ascribed to its influence by Mr. Greenough, without affording an anomaly in

nature far greater even than that which it is brought to explain. The main difficulty which seems to strike all those who have hitherto considered the subject, appears to lie in the method of getting rid of the waters of the Deluge. Many will grant you that they came, if you can show how they departed. Amidst all the conjectures that have been offered on this point, sufficient stress does not appear to have been laid upon the idea, that they may not have departed; but that the waters which "were *increased* greatly upon the earth" are still with us. I shall offer a few remarks upon this subject, rather with a view to promote future inquiry, than with the wish to propose a new hypothesis. I shall assume that the flood which we are informed prevailed for 150 days, consisted of waters *at that time added* to the earth, and leave to the future consideration of geologists, whether the supposition of their having been partly absorbed by the solid portion of the earth is not of itself a cause sufficient to explain the present state of the surface of our planet.



Suppose the original level of the surface of the ocean to have been the line A, and an increase of waters to have raised the surface from A to B, sufficient to cover the tops of the highest mountains; I would ask whether, if the increase were rather sudden as it is stated to have been, we may not imagine that a considerable depression below the highest level would afterwards take place, owing to those solid portions of the earth which were not originally covered, becoming saturated with moisture; and thus, after a certain lapse of time, the surface of the ocean might rest at C, leaving the higher summits of the old continents again exposed. To decide whether or not such may have been the case will of course require that future observations should be made with this object in view. There are, however, certain facts already noticed in geology which tend to show that an increase of elevation above the original surface of the ocean has actually taken place; such as peat land, containing vast numbers of trees, which are found in some situations extending under the bed of the ocean, and whose destruction appears to have been coeval with the Deluge. Also the swampy condition of large tracts of fen country, which are now incapable of producing timber, but in which immense quantities of the largest growth are found buried in a sound state. If it should be objected that the mass of waters brought upon the earth was far too great to admit of the supposition of their having been absorbed to an

extent sufficient to allow for the present altitude of the highest mountains above the surface of the ocean, I reply that this must be a subject for future observation ; but I think this difficulty, which at first sight appears so startling, will diminish considerably by considering what would be the quantity of water sufficient to cover the tops of the highest mountains at this present moment. The greatest altitude of any known mountain is about five miles, which is but trifling compared with the radius of the earth, which is above 4000 miles. Let a person take a three-inch globe in his hand, and consider how thin would be the film of water sufficient to cover the particles of fine dust which are attached to its surface. It should seem also that those portions of the earth which were partly saturated by moisture before the Deluge would absorb a still greater quantity upon the rise of the ocean, and thus a further diminution might be accounted for. Suppose a basin made of plaster of paris, chalk, or any other porous material, to be partly filled with water, the sides will immediately imbibe a certain portion, and the surface will fall. When the water has reached its greatest depression, fill the basin, and you will find the sides still capable of absorbing an additional quantity, and the surface will not long remain on a level with the rim. I need scarcely observe that the earth appears to be in every part more or less saturated with water. The most solid rocks contain it in great abundance, and the operations of the miner are too frequently impeded by its presence ; but I believe that few, if any, observations have hitherto been made with the view of obtaining an estimate of the mean quantity at different depths, or in different descriptions of rock.

This view of the subject perfectly coincides with the account given in Genesis of the gradual manner in which the waters subsided. "And the waters returned from off the earth *continually*." " And the waters decreased *continually* until the tenth month." This would hardly have been the case if we are to suppose with some, that there were large empty receptacles prepared for them towards the centre of the earth, into which they suddenly retired.

I should scarcely venture to allude to the manner in which we may suppose that the waters were *brought* upon the earth, but that I wish to observe upon some other phenomena connected with the supposition of their having been added to the earth's surface, or, in the language of scripture, " *increased* upon the face of the earth."

Observation appears to have established that the rise of the diluvian waters was *gradual*, and that with respect to the present surface of the land they came in *descending* torrents. And this agrees distinctly with one part of revelation, which states, that " the windows of heaven were opened, and the *rain* was upon the earth forty days and forty nights," " and the waters *prevailed*,

and were *increased* greatly upon the earth." All which seems to imply an extraneous supply of water; for although the atmosphere at all times contains a certain quantity of aqueous vapour, yet this would not be sufficient to answer the demand. We know that the weight of the whole atmosphere is equivalent only to a depth of water of about 34 feet, and this is made up of atmospheric air, water, and different gaseous mixtures. We must, therefore, look for some other cause, which has ceased to operate since the supply was furnished; and here of course nothing but conjecture can be offered. With many of my predecessors in this department, I must have recourse to one of those bodies which have so often been considered as the probable cause of the Deluge, though the mode in which they have been supposed to operate in effecting that event has as often been refuted or ridiculed. But before we attempt to enlist so mysterious an agent into our service, let us inquire what it is we actually know respecting the nature of comets.

Of this we can judge only from the astronomical and optical pheuomena which they present. It is certain that they are material substances, and it appears universally conjectured by the most accurate observers, that they are in great part, if not wholly, composed of aqueous vapour. Some comets present a nucleus encircled by this vapour; others have no nucleus at all. As they approach the sun, they become brighter; the luminous train or tail, when it exists, becomes enlarged and more brilliant, and when the comets have arrived at their perihelion, their lustre is sometimes found to exceed that of the planets. In their retreat from the sun, these phenomena are reversed, till at length the light reflected from them is too trifling to be any longer visible. Although the opinions which have been promulgated concerning the tails of comets differ materially with respect to what may be the nature of their substance, yet they are all compatible with the idea of their nuclei being composed of aqueous particles. One opinion is that these tails are the light of the sun refracted through the comet acting like a transparent lens; but this idea seems to have been satisfactorily refuted by subsequent observation. Others suppose them to be the vapour of the comet, either driven behind it by the impulse of the sun's rays, or raised by the heat of the sun; the latter opinion, which was held by Sir Isaac Newton, seems also compatible with the idea of the comet transmitting the rays of light, since the heat would be greatest, and consequently the vapours lightest, along the train of light reaching from the nucleus to the focus of this astronomical lens. However this may be, let it be granted as highly probable, that some comets are composed of aqueous particles, which at a distance from the sun will probably concrete into the form of a globe of ice, and on approaching him will either be wholly or in part converted into vapour. What will be the effect of such a comet approaching within the sphere of the earth's attraction?

Before replying to this question, I will ask what has been the fate of the comet of 1770, whose periodic time was not greater than five years and a half, and which could never wander so far from the sun as to get beyond the orbit of Saturn? Yet this has not been since that time, and no solution of the cause of its disappearance appears so probable as that offered by Dr. Brewster, "that it now exists under the form of those enormous atmospheres which accompany Ceres and Pallas."

If it be too much to suppose that the nucleus of such a comet, though composed of aqueous particles, would fall to the earth, we may, perhaps, conjecture that a portion of its nebulous train becoming entangled in our atmosphere would be attracted to the earth, and descend in the form of rain upon every portion of its surface successively, as the earth turned upon its axis. This conjecture as to the mode of supply is here mentioned, as I before stated, with the view of promoting another inquiry upon a point which seems to be pretty well established in geology, viz. that the mean temperature of the earth's surface, at least in these latitudes, has been very sensibly diminished ever since the Deluge. If the mean temperature of the earth's surface depend upon the distance of the centre of the earth from the surface of the ocean, then the increase of waters brought by the deluge would, by increasing the radius of the earth, produce the phenomenon which has been observed.

It may also be proposed as a subject of inquiry, what would be the effect produced upon the atmosphere by increasing the proportion of aqueous surface to that of the dry land. Would the atmosphere become more highly charged with aqueous vapour, and cause a greater quantity of rain to fall annually? We know that some of the planets possess an atmosphere of extreme purity, and that others apparently have none. Among the former number is the moon. Now it is remarkable that the latest discoveries lead us to suppose that the moon possesses no seas, though there are large indentations on her surface which would speedily become such, were she inundated by a Deluge.

How far such investigations as these may tend to confirm the history of the rainbow having been first seen after the Deluge, or, in other words, the non-existence of rain previous to that event, I leave to the inquiry of meteorologists, requesting them to bear in mind that the only account we have of the method by which the earth was refreshed before the Deluge is, that "there went up a mist from the earth and watered the whole face of the ground;" "For the Lord God had not caused it to rain upon the earth."

I remain, Sir, your obedient servant,

J. S. Henslow.

Article V.

Facts, Observations, and Conjectures, relative to the Generation of the Opossum of North America. In a Letter from Prof. Barton to Mons. Roume, of Paris.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Penlerrgare, Sept.* 3, 1823.

I received from its author, the late Prof. Barton of Philadelphia, the enclosed printed copy of a letter which I have never elsewhere met with, and it relates some circumstances which have not been noticed by Sir Everard Home in his valuable observations on the generation of the marsupialia in the Philosophical Transactions for 1808, 1810, and 1819.

The fossil remains of a species of didelphis are said to be not unfrequently found in the Stonesfield slate, and I know of no other animal belonging to either of the secondary or any older formation which possesses the smallest claim to be called viviparous, nor does even this family in its mode of generation, appear to be more than one of those links which connect the higher order of viviparous with oviparous animals.

In this point of view the Professor's letter becomes interesting not only to the zoologist, but in some degree to the geologist also; and I, therefore, offer it to you for insertion in the *Annals of Philosophy*. I am, dear Sir, yours truly,

L. W. Dillwyn.

DEAR SIR, *Philadelphia.*

In looking over my list of correspondents, I find that I am indebted to you a letter. I cannot think of writing a mere formal letter of apology, for my long silence; and, therefore, I shall contrive to send you something that may, at least, *amuse* you.

You and I have often talked together, and speculated, about the generation of the Opossum of North America (the Virginian Opossum of Pennant; my Didelphis *Woapink*).* I think I

* There is not a little confusion concerning the nomenclature of the different species of Didelphis, in the writings of Linnæus, Gmelin, and other naturalists. See the articles "Didelphis marsupialis," and "D. Opossum," in the *Systema Naturæ*, as published by Linnæus himself, and by Gmelin. I have, therefore, thought it most advisable to impose a new and more determinate name upon the animal, which has been the subject of my experiments. The specific name of *marsupialis* is not very happily applied to any particular species of Didelphis, since most of the species of this singular genus are furnished with the *marsupium*, or abdominal sack. I object to Dr. Shaw's specific name, *Virginica* (taken from Mr. Pennant), because it implies, that our Opossum is restricted to, or especially common in, Virginia; whereas this animal is nearly equally common in every part of the United States (east of the Missisippi), from the latitude of 40 to that of 25, and even much further south. The name *Woapink*, which

informed you, when I had the pleasure of seeing you in Philadelphia, that I had, for several years, been engaged in an extensive series of experiments and observations relative to this curious animal; this "prodigiosum animal," as Benzoe calls it.* The result of my inquiries will be communicated to the public in two memoirs, the second (and most difficult) of which is nearly finished.

In the first of these memoirs, I shall detail, at length, the general natural history of the animal; examine its place in the system; its food; its manners; its geographical range through the continent, &c. I shall also particularly notice the periods of the intercourse of the sexes, and shall pursue the female through the whole progress of what I call the *uterine* gestation, which comprehends a period of between twenty-two and twenty-six days.

The other memoir will commence with the second term of gestation, which I call the *marsupial* gestation. This, which dates its beginning from the first reception of the embryons from the uterus, into the marsupium, *bourse*, or pouch, is much longer than the uterine gestation, and comprehends, even in a physiological point of view, by far the most interesting era in the history of the animal. I have been so fortunate as to ascertain the size and weight of several embryons *immediately* after their exclusion from the uterus. One of them weighed only one grain! The weight of each of the six other young ones was but little more than this.

The young opossums, unformed and perfectly sightless as they are at this period, *find* their way to the teats by the power of an invariable, a *determinate* instinct, which may, surely, be considered as one of the most wonderful that is furnished to us by the science of natural history.† In this new *domicilium*, they continue for about fifty days; that is, until they attain the size of a common house-mouse (Mus musculus), when they begin to leave the teats *occasionally*, but return to them again, until they are nearly of the size of rats (Mus rattus), at which time they seem to be no longer *necessarily* supported by the milk of the mother, but eat meat and vegetables of various kinds.

The female Didelphis Woapink sometimes produces sixteen young ones at a birth. I have actually seen this number *attached*

I have chosen, signifies "white face." I should, perhaps, have preferred the Tuscarora or Cheerake names, *Cheera*, or *Seequa*, but that I know not the precise meaning of these appellations. I may add, in this place, that the specific name of "*dorsigera*," which Linnæus has applied to another species of didelphis (the Merian opossum of Pennant) is likewise exceptionable; for I have discovered, that my Didelphis Woapink often carries her young ones upon her back.

* Lib. ii. p. 215.

† It is not true, as has been often asserted, that the mother, with her paws, *puts* the young ones into the pouch. In my first memoir, I shall show, to the satisfaction of every one, that the common opinions on this subject are altogether erroneous.

to the teats; but never a greater number.* When they are first excluded from the uterus, they are not only very small, but very obscurely shaped. The place of the future eyes is merely marked by two pale-bluish specks: we see no ears; in short, the animal is a mere mishaped embryon. Its mouth, which is afterwards to become very large, is, at first, a minute hole, nearly of a triangular form, and just of a sufficient size to receive the teat, to which the little creature adheres so firmly, that it is scarcely matter of surprise, that Beverley † and other writers have asserted, that the young are originally produced in the marsupium, where they grow fast to the teats; an opinion very generally adopted in many parts of the United States.

It is not true, that the young cannot be detached from the mother, without the loss of blood. I can assert the contrary from many experiments, made upon embryons weighing nine grains, and upwards. I have fully satisfied myself as to all the various circumstances, both in the structure and in the exertions of the minute animal, which enable it, while yet a mere speck, as it were, of living matter, to cling so firmly to the fountain of its support.

It is truly an interesting task to pursue the various steps in the progressive evolution of the parts of the young opossum, while in the marsupium, and especially so long as it is *necessarily* attached to the teat. It is natural to suppose that the all-careful hand of Nature first evolves those parts, which are the most immediately important to the animal. In this supposition we are not mistaken. It is a long time before the embryon has any occasion for the senses of sight and hearing: but a mouth and the powers of deglutition, as well as of breathing, are necessary to it, *immediately* after its exclusion from the uterus. Accordingly, its mouth and nostrils are open; and, for a long time, all the air which it respires is received through, and passes out of, the latter channels. The stomach seems to perform its digestive office in the embryon immediately after its first attachment to the teat; ‡ and the wonderful little didelphis is by no means the inanimate or the passive being some physiologists and naturalists have represented it.§

* I have been informed, that female opossums have been seen with more than sixteen young ones, of the *same birth.* I cannot, however, place implicit dependence upon this information, especially as I have never seen an opossum with *more* than sixteen teats.

† "The young ones (says this writer) are bred in this false belly, without ever being within the true one. They are formed at the teat, and there they grow for several weeks together into perfect shape," &c.—(The History of Virginia, &c. p. 136. London, 1722.

‡ In an opossum weighing only forty-one grains, I have seen the stomach very considerably distended with a white matter, or milk. But the milk that is afforded to the embryons, for a few days after their first reception into the marsupium, is nearly pellucid, or transparent.

§ Mr. Pennant says they adhere to the teats "as if they were inanimate, till they arrive at a degree of perfection in shape, and attain sight, strength, and hair; after which they undergo a sort of second birth."—(Arctic Zoology, vol. i. p. 84.)

The toes of the fore-feet of the new-born embryon opossum are furnished with sharp and hard nails, or claws; but this is not the case with the hind-feet. The latter are, for some weeks, of little use to the animal; but by means of the former it is enabled to cling most firmly to the teat; and especially to the hair in the marsupium immediately around the teat. I cannot suppose, with the respectable Mr. E. Home, of London, that the viscous fluid which surrounds the body of the embryon, when it is first excluded from the uterus, is of *any* service in facilitating its attachment to the teat.*

There is one instance of the evolution of the parts of the embryon-opossum, which has greatly surprised me, and seems, with many other facts, to show, that Nature will, for a long time at least, confound our endeavours to unravel her *rete mirabile* of final causes. In an embryon-opossum, weighing only sixty or eighty grains, and entirely destitute of the senses of sight and hearing, you may observe, with the naked eye, the marsupium of the female *distinctly* formed, and even count the number of the teats.

The humane and ingenious conjecture of Buffon, concerning the preservation of human embryons, or at least fetus, far from being arrived at their last stage of growth, has received some confirmation from my experiments: † but I cannot at present detail these experiments. I shall only observe, that an opossum-embryon, or fetus, which weighed sixty-seven grains, lived upwards of thirty hours after I had detached it from the teat. Another, which weighed 116 grains, lived thirty-eight hours, at which time I killed it, by putting it into spirits.

At the end of about fifty or fifty-two days, from its first reception into the pouch (the period varies somewhat, even among the different individuals of the same birth), the eyes of the young begin to open. At this period, and for a short time before, it is capable of *retaking* the teat, after having been separated from it by the hand, or otherwise.

The growth of the young opossum while in the marsupium, and under the immediate care of its mother, is pretty rapid. I have found that the same embryon has increased in weight 531 grains in sixty days; that is, at the rate of almost nine grains

* Speaking of the kanguroo, Mr. Home says, "It would seem probable, that the mouth of the fœtus is originally attached to the nipple by means of the gelatinous substance contained in the uterus."—(Observations on the Mode of Generation of the kanguroo.)

† "Personne n'a observé la durée de la gestation de ces animaux, que nous présumons être beaucoup plus courte que dans les autres; et comme c'est un exemple singulier dans la Nature que cette exclusion précoce, nous exhortons ceux qui sont à portée de voir des sarigues vivans dans leur pays natal, de tâcher de savoir combien les femelles portent de temps, et combien de temps encore après la naissance les petits restent attachés a la mamelle avant que de s'en séparer; *cette observation, curieuse par elle-même, pourroit devenir utile, en nous indiquant peut-être quelque moyen de conserver la vie aux enfans venus avant le terme.*"—(Histoire Naturelle, &c. tom. xxi. p. 171, 172. A Paris, 1765.

daily. But, as you may readily imagine, its increment, in bulk and weight, is much greater one day than another. The animal attains to nearly its full growth in about five months; but never, I believe (in our latitudes, I mean), procreates the first year of its existence.

Possibly I have been relating nothing but what is familiarly known to you. The following fact, however, will, I flatter myself, be entirely *new* to you; and if the relation of it should give you half the pleasure that the discovery of it did me, I am persuaded, that this letter will not be altogether unacceptable to you.

On the 14th of May, I purchased a female opossum, with seven young ones. They were at this time about the size of rats, two-thirds grown; and subsisted partly upon their mother's milk, and partly upon meats and vegetables. Of course, the period of their *necessary* connexion with the mother was at an end.

On the 21st of the month, that is, at the expiration of seven *complete* days, upon looking into the box which contained the animal, I found that the mother had just excluded from her uterus seven embryons, the smallest of which scarcely weighed one grain; another barely two grains; and the remaining five (taken together) exactly seven grains.

You, my dear Sir, who are by no means a stranger to the enthusiasm that is inspired by the contemplation and study of Nature, will readily imagine what were my sensations on the discovery of this *unexpected* new family of didelphides. The fact, which I was so fortunate as to witness, is, in my opinion, one of the most interesting in the whole science of zoology; and so far as I know, it has never been noticed by any naturalist but myself.

You will, I doubt not, immediately attach to this fact its proper and full value. We are no longer, it appears to me, at a loss to comprehend the final intention of Nature in furnishing the opossum with a pouch for the reception of the tender embryons, excluded, as we have seen they are, from the uterus, in a very unformed state. Nature has determined that the female didelphis shall produce, at least, two litters of young ones, in the course of the same year. Superfetation (I should, perhaps, in strict propriety, say *uterine* superfetation) is wholly incompatible with the established laws of the economy of the didelphis. But Nature, always provident, wastes no time. While, therefore, the first litter of young opossums are fast approaching to their adult or more independent state, the mother accepts the ardour of the male; she is impregnated; and after a gestation which is not, I think, remarkably short, if we consider the small size of the embryons when they are excluded from the uterus,* the

* Buffon, in the passage which I have quoted from his work, has very properly observed, that the uterine gestation of his Sarigue is very short: short, indeed, when

marsupium is destined to perform the office of a second, I was going to say a more important, uterus; just at the time when the first litter have attained such a size, that they are no longer (one or two of them at the utmost) capable of taking refuge in her pouch; and when, being now provided with teeth, and the requisite strength, they are not *necessarily* dependant upon their mother.

But even after the second litter has been received into the marsupium, the young of the first litter, if any of them be living, still continue with the mother, who does not yet withdraw from them her useful attentions and assistance. They are no longer indeed permitted to take the milk secreted by her breasts; but she sedulously watches them, and even conveys them, while they cling to her back and tail, for considerable distances through the woods, &c.

* * * * *
* * * *

But it is time to put an end to this long letter. Believe me, I shall be truly glad if it afford you any information or amusement.

With the genuine regard of a naturalist, I remain, my dear Sir, your friend, &c. Benjamin Smith Barton.

Article VI.

Astronomical Observations, 1823. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Oct. 2.	Immersion of Jupiter's second satellite	12h	11′	7″	Mean Time at Bushey.
		12	12	28	Mean Time at Greenwich.
Oct. 2.	Immersion of Jupiter's first satellite	17	26	57	Mean Time at Bushey.
		17	28	18	Mean Time at Greenwich.
Oct. 9.	Immersion of Jupiter's second satellite	14	47	28	Mean Time at Bushey.
		14	48	49	Mean Time at Greenwich.
Oct. 11.	Immersion of Jupiter's first satellite	13	48	38	Mean Time at Bushey.
		13	49	59	Mean Time at Greenwich.
Oct. 16.	Immersion of Jupiter's first satellite	17	23	27	Mean Time at Bushey.
		17	24	48	Mean Time at Greenwich.

we compare this first gestation with that of the marsupium. But I have shown, that the female didelphis carries her young *in utero* between twenty-two and twenty-six days, which is no inconsiderable period, if we reflect on the very small size (sometimes *less* than one grain) of the embryons, when they are dislodged from the uterus: for the weight of our female opossum is often, at least, 18 lbs.

Article VII.

An Appendix to the Abstract of M. Ramond's Instructions for Barometrical Measurements. By Baden Powell, MA. of Oriel College, Oxford.

(*Concluded from* p. 274.)

In order to render more complete the foregoing compendium, and as some readers may wish for an account of the principles on which the formula is constructed, it may not be improper here to add for their convenience a brief explanation of it, together with some remarks on other points connected with the subject.

I. *Outline of the Demonstration of the Formula.*

M. Biot, in the small tract prefixed to his "Tables Barometriques Portatives," has given at large the demonstration of a formula which differs from the present only in some very slight modifications. I shall, therefore, do no more than present a sketch of his elegant investigation, the principles of which may, perhaps, be made sufficiently intelligible, without following him through all the detail of his analytical processes. The reader who is desirous of fully appreciating their beauty is referred to the original.

As I here propose only to give a mere outline of the investigation of the formula, it will be superfluous to go through the elementary proof of the general theorem, which establishes the relation between pressure and elevation. We may set out by assuming that the difference of elevation, $z = \frac{M}{C} \log. \left(\frac{h}{H}\right)$, M being the modulus of the common system of logarithms, and C a coefficient involving the various corrections.

(1.) Our object is to discover the coefficient C. This M. Biot proceeds to do in the following manner:*—If we represent by δ the density of the air under the pressure h, that of mercury being unity, we have $\delta = C\,h$, and $\frac{\delta}{h} = C$. We may obtain, therefore, the value of C, if we have, by very exact experiments, the ratio of the densities of air and mercury, under a given pressure of the atmosphere.

This ratio is not the same in all countries; for in all countries the weight of bodies has not the same intensity, as we learn from experiments on the pendulum, and the ratio $\frac{\delta}{h}$ varies with the intensity of gravity. Indeed δ is the density of the air under

* Mesures Barometriques, p. 7.

a given pressure, for instance, 29·921 inches, but according as the intensity of gravity is greater or less, a column of mercury of the constant height of 29·921 inches will weigh more or less; consequently air subjected to this pressure will be more or less compressed. Now by experiments with the pendulum in different latitudes, we find that calling the force of gravity in lat. 45°, unity, in a latitude ψ, it will be expressed by $1 - 0{\cdot}0028371 \,.\, \cos. 2\psi$.* The density δ being proportional to the weight will vary in the same ratio; that is to say, calling it δ in lat. 45°, and under the pressure h, it will become for any other latitude, and under a column of mercury of the same height, $\delta\,[1-0{\cdot}0028371 \,.\, \cos. 2\psi]$. The coefficient C, which expresses the ratio of the density to the height of the barometric column, ought to vary in the same proportion, and consequently becomes $C\,[1-0{\cdot}0028371 \,.\, \cos. 2\psi]$, which being substituted in the value of z, gives $z = \frac{M}{C\,.\,[1-0{\cdot}0028371\,.\,\cos. 2\psi]} \,.\, \log. \left(\frac{h}{H}\right)$: in this way it will be sufficient to find the coefficient $\frac{M}{C\,.\,[1-0{\cdot}0028371\,.\,\cos. 2\psi]}$ by experiment for a given latitude; for thus, ψ being known, we shall know also $\frac{M}{C}$; and the formula becomes applicable to all possible latitudes. The formula may be rendered more convenient by causing the denominator to disappear, which is easily done; for the fraction $\frac{1}{1-0{\cdot}002837\,.\,\cos. 2\psi}$ being developed in a series by division, becomes $1 + 0{\cdot}002837 \cos. 2\psi + 0{\cdot}00000804857, \cos.^2 2\psi + \ldots\ldots$ or simply $1 + 0{\cdot}002837 \cos. 2\psi$ by confining ourselves to the first term, which is alone of sensible magnitude. Thus we shall have $z = \frac{M}{C}[1 + 0{\cdot}002837 \cos. 2\psi] \log. \left(\frac{h}{H}\right)$.

(2.) Thus far we have supposed that the value of the coefficient C or $\frac{\delta}{h}$ is the same in all the strata of the column of air; but it is not so in nature; and many causes tend to make this ratio vary. The principal cause is the inequality of the temperature of the strata; for the elasticity of air is augmented by heat, so that with a less density, it can support an equal column of mercury, which makes the ratio $\frac{\delta}{h}$, or C, vary.

This ratio also varies according to the greater or less quantity of aqueous vapour which is found suspended in the different strata; for this vapour weighs less than dry air of equal elastic

* This expression is deduced from one given by Laplace, Mec. Cel. b. 10.—(See M. Ramond's First Memoir, Part II. p. 16.)

force, so that its presence in the different strata renders them proportionally capable of sustaining with a less density an equal column of mercury.

Lastly, the decrease of gravity as we recede further from the centre of the earth is another cause of the change; for by this decrease a column of mercury whose length is h, weighs so much the less, as we recede from the centre: if it weigh less, it compresses less the strata of air into which it is carried: thus the ratio of their density to the length of the column of mercury, or $\frac{\delta}{h}$, is no longer the same for these strata as for those which are below. If all other circumstances are alike, the densities of the strata of air which these columns compress will be likewise proportional to them. The ratio $\frac{\delta}{h}$, or C, therefore, ought to vary from one stratum to another proportionally to the force g.

(3.) The amount of each of these corrections may be calculated on the following principles:—

First, the action of temperature. From the influence of this cause, a mass of air whose volume is 1 at zero (centig.) becomes at t degrees, $1 + t \,.\, 0{\cdot}00375$, the barometrical pressure remaining the same. Under a constant pressure, the densities of this mass are reciprocally as the volumes, and, therefore, if the density at zero be 1, the density at t degrees will be $\frac{1}{1 + t \,.\, 0{\cdot}00375}$ under a constant pressure; the ratio $\frac{\delta}{h}$, or C, must, therefore, vary proportionally to this quantity.

Secondly, the influence of aqueous vapour. According to the experiments of De Saussure and Watt, the weight of this vapour is to that of air as 10 to 14, while their elastic forces and temperatures are the same; that is to say, while the air and the vapour being at the same temperature, sustain equal columns of mercury. The substitution, therefore, of this vapour in the strata of the air, renders them specifically lighter without diminishing their elastic force. To obtain the value of this effect, let h be the barometrical pressure which supports a certain stratum of air: let us call F the elastic force of the aqueous vapour contained in it; that is to say, the part of the barometrical pressure which the vapour sustains. The whole weight of the stratum may be considered as composed of two parts, viz. of a certain quantity of vapour whose elastic force is F, and of a certain quantity of atmospheric air perfectly dry, whose elastic force is $h - \mathrm{F}$. Let p be the whole weight of the stratum, if it were composed entirely of dry air under the pressure h. The weight of the same volume of dry air under the pressure $h - \mathrm{F}$ will be $p\,\frac{(h - \mathrm{F})}{h}$. The weight of the same volume under the pressure F will be $\frac{p\mathrm{F}}{h}$. Lastly, if this volume remaining always

under the pressure F, were composed entirely of aqueous vapour, its weight would be $\frac{10}{14}$ of the former; that is to say, $\frac{10}{14}\frac{pF}{h}$. Now we know by very decisive experiments that in a mixture of vapour and air, which has attained a state of stable equilibrium, these two fluids are uniformly diffused throughout the whole space which they occupy. Thus the weight of the mixture in the preceding proportions will be equal to the sum of the weights of the air and vapour which occupy the given space under the pressures $h - F$ and F; that is to say, that this weight will be $p \cdot \frac{h-F}{h} + \frac{10}{14}\frac{pF}{h}$, or simply $p \cdot \frac{h - \frac{2}{7}F}{h}$. Now before the introduction of the vapour, the weight of the same volume of dry air submitted to the same pressure h, would be represented by p. The densities being proportional to the weights, if δ represent the density of the stratum in the dry state, the density in the moist state will become $\delta \cdot \frac{\left(h - \frac{2}{7}F\right)}{h}$, or $\delta \cdot \left[1 - \frac{2}{7}\frac{F}{h}\right]$ the pressure remaining the same. Thus we see that the introduction of aqueous vapour in the strata of air makes the ratio $\frac{\delta}{h}$, or C, vary proportionally to $\left(1 - \frac{2}{7}\frac{F}{h}\right)$.

The tension F is always very small at those temperatures at which barometrical observations are commonly made. Its value in metres for the point of extreme saturation may be calculated from a formula, given by Laplace, from the experiments of Dalton; whence we find,

At 0° centigrade F = 0·005122 metre; (= 0·20165 inch.)
At 30° centigrade F = 0·031690 metre: (= 1·24765 inch.)

and within these limits, which are nearly those of barometrical observations, the increase of F may be sufficiently well represented by arithmetical progression, and will be

$$F = 0{\cdot}005122\,\text{m.} + 0{\cdot}0008649\,\text{m.}\ t \ (= 0{\cdot}20165\,\text{in.} + 0{\cdot}03304\ t)\ \text{in.}$$

t being the temperature centigrade. Although this formula is not rigidly accurate, it is sufficiently so in practice on account of the little effect which it has on the observed heights.

But before it can be applied to the state of the atmosphere, it requires to be modified. It relates to the point of extreme saturation at which the atmosphere is scarcely ever found; and consequently the value of F will almost always be rather greater than the truth. No general determination can be given of the quantity of vapour suspended in the atmosphere. This quantity is extremely variable on different days; it varies even from one stratum to another in a manner very irregular, and often abrupt, as we see on mountains where strata very little charged with

vapour succeed others which are at the maximum of humidity. However, setting aside these extraordinary circumstances, every thing leads us to believe that we shall follow nature most closely if we avoid these extreme cases; and thus what seems most simple is to take for the expression for F in the atmosphere the half of the value which corresponds to the point of extreme humidity; that is to say,

$$F = 0{\cdot}002561 \text{ metre} + t \,.\, 0{\cdot}00043245 \text{ metre}$$
$$(= 0{\cdot}10082 \text{ inch} \quad + t \,.\, 0{\cdot}01652) \text{ inch}.$$

In substituting this value in the expression for the coefficient C, it must be multiplied by the variable factor $\frac{2}{7\,h}$, but on account of the minuteness of this correction, and also on account of the small difference in the values of h within the limits of ordinary measurements, it will suffice to put for h, the constant value 0·76 m. = 29·921 in. which is the mean pressure at the level of the sea. This substitution will possess also the advantage of giving a less correction for the humidity in the higher strata of the column, which agrees with nature; for the humidity of these strata generally diminishes in proportion as we ascend, and sometimes the most elevated are extremely dry. Adopting this simplification, we have

$$1 - \frac{2\,F}{7\,h} = 1 - \frac{2}{7\,.\,0{\cdot}76\,\text{m.}}\,[0{\cdot}002561 \text{ m.} + t \,.\, 0{\cdot}00043245 \text{ in.}] =$$
$$1 - {\cdot}0009628 - {\cdot}0001626 \,.\, t.$$

Without sensible error this expression may be put under the following form, $[1 - {\cdot}0009628]\,[1 - {\cdot}0001627 \,.\, t]$ which gives

$$C = \frac{A\,[1 - {\cdot}0009628] \,.\, g\,[1 - {\cdot}0001627 \,.\, t]}{1 + t \,.\, 00375}.$$

The factor, depending on t, which is found in the numerator, may be combined with that which arises from the temperature. On account of the smallness of the coefficient ·0001627, we may without sensible error substitute $\frac{1}{1 + {\cdot}0001627 \,.\, t}$ in the place of $1 - {\cdot}0001627 \,.\, t$.

Thus we have in the denominator the product $[1 + {\cdot}0001627 \,.\, t] \,.\, [1 + t \,.\, 0{\cdot}00375]$. In performing the multiplication we may neglect the product of ${\cdot}0001627 \times {\cdot}00375$: and thus it becomes $[1 + {\cdot}0039127 \,.\, t]$. The coefficient of t in this result differs so little from ·004, or $\frac{1}{250}$, that we may, without fear of error, substitute for it this last value, which will simplify the calculation. We have, therefore,

$$C = \frac{A\,[1 - {\cdot}0009628] \,.\, g}{[1 + t \,.\, 0{\cdot}004]}.$$

Thirdly, the variation of the force of gravity must affect both the coefficient or ratio of densities of air and mercury, and also the observed heights of the column of mercury at the two

extremes of the elevation. If we call the force of gravity at the surface g, and that at the height z, g_{II}, it is obvious that $\frac{h}{H}$ must be reduced in the ratio of $\frac{g_I}{g_{II}}$, or we must take log. $\left(\frac{h}{H}\right)$ + log. $\left(\frac{g_I}{g_{II}}\right)$. The last term, from considering the law of distance, may be converted into 2 log. $\left(1 + \frac{z}{a}\right)$: (a being the mean radius of the earth, which may be substituted as very nearly the distance of the lower station from the centre; and instead of $\frac{g_I}{g_{II}}$ putting $\frac{(a+z)^2}{a^2}$.)

This last expression may be made use of in applying the correction for gravity to the coefficient C. It will be sufficiently accurate to take the expression for the force of gravity at the mean elevation, which will be $g_I \frac{a^2}{\left(a + \frac{z}{2}\right)^2}$. And dividing by a^2, and neglecting all powers above the first, this becomes nearly $g_I \cdot \frac{1}{\left(1 + \frac{z}{a}\right)}$. Taking also, instead of the indeterminate t, the mean temperature of the air at the two stations, we shall have

$$C = \frac{A\,(1 - \cdot 0009628)\,g_I}{\left(1 + \frac{2\,t + t'}{1000}\right) \cdot \left(1 + \frac{z}{a}\right)}.$$

These expressions for the diminution of gravity are deduced by M. Biot through a series of analytical forms, in which he traces the effects of the force in question from one stratum of air to another, and then effects a summation. This is in accordance with the elegant method he has adopted throughout the whole investigation. In giving this outline, I have merely attempted to state in general terms the grounds upon which each correctional expression may be deduced; but for the details of the analysis, the student is referred to M. Biot's tract.

Fourthly, the formula now stands thus,

$$z = \frac{M}{A\,(1 - \cdot 0009628)\,g_I}\,(1 + \cdot 0028371 \,.\, \cos. 2\,\psi)\left(1 + \frac{2\,t + t'}{1000}\right)$$
$$\left(\log. \left(\frac{h}{H}\right) + 2 \log. \left(1 + \frac{z}{a}\right)\ \right)\ \left(1 + \frac{z}{a}\right).$$

Then developing 2 log. $\left(1 + \frac{z}{a}\right)$, and keeping to the first power (since z is very small in respect to a), it becomes $\frac{z}{M\,a}$. Then multiplying the last two factors (keeping to first power), we get log. $\left(\frac{h}{H}\right) + \frac{z}{a}\left(\log. \frac{h}{H} + \frac{2}{M}\right)$; whence (since $\frac{2}{M}$

$= \cdot 868589\big)$ the last two factors become log. $\frac{h}{H}$

$$\left(1 + \frac{\left(\log. \frac{h}{H} + \cdot 868589\right) \frac{z}{a}}{\log. \frac{h}{H}}\right)$$ which gives exactly M. Ramond's formula, excepting that the constant coefficient remains to be determined.

This, M. Biot now proceeds to investigate, by taking as at first δ = density of dry air, that of mercury being 1, under the pressure h, at temp. t, latitude ψ, intensity of gravity g. Then we have*

$$\delta = \frac{A\,(1 - \cdot 0028371\,.\,\cos.\,2\,\psi)\,g\,h}{1 + t\,.\,0\cdot 00375}.$$

The most simple means of finding A is to weigh with great exactness known volumes of air and mercury under a given pressure and temperature, in a place whose latitude and elevation are known. This experiment M. Biot informs us has been tried at Paris with the greatest care by Arago and himself. They found that at the temperature of melting ice, and under the pressure of $0\cdot 76$ m. $\delta = \frac{1}{10463\cdot 0}$; whence $A =$

$\frac{1}{10463\,.\,g\,(1 - \cdot 0028371\,.\,\cos.\,2\,\psi)\,0\cdot 76\text{ m.}}$, ψ being the latitude of Paris. Consequently representing by M the modulus of the logarithmic tables, or $2\cdot 30258509$, the coefficient of the barometric formula, or $\frac{M}{A\,g_1}$, will become

$$\frac{M}{A\,g_1} = 10463\,(1 - \cdot 0028371\,.\,\cos.\,2\,\psi)\,0\cdot 76\text{ m.}\,M\,.\,\frac{g}{g_1}.$$

If we reduce this value into numbers taking $\psi = 48°\ 50'\ 14''$, which is the latitude of the Observatory, we find $\frac{M}{A\,g_1} =$ $18316\cdot 82$ m. $\frac{g}{g_1}$, and consequently $\frac{M}{A\,g_1\,(1 - \cdot 0009628)} =$ $18334\cdot 46$ m. $\frac{g}{g_1}$. Let r be the elevation of the inferior station above the level of the sea, $a + r$ will be its distance from the centre of the earth. The elevation of the place where they tried their experiments on the weight of air and mercury may be assumed at 60 metres above the level of the sea: its distance from the centre of the earth in metres will, therefore, be $a + 60$. Thus the ratio of the weights $\frac{g}{g_1} = \frac{(a + r)^2}{(a + 60)^2}$, an expression which reduces itself to $\left(1 - \frac{120}{a}\right)\left(1 + \frac{2\,r}{a}\right)$, developing the two

* Mesures Barometriques, p. 23.

squares, and confining ourselves to the first powers of $\frac{60}{a}$, and of $\frac{r}{a}$.

The first factor $1 - \frac{120}{a}$ may be reduced into numbers taking $a = 6366198$ m. as we before assumed; it diminishes the barometric coefficient by 0·35 m. which gives $\frac{M}{A g_1} = 18334{\cdot}11$ m. $\left(1 + \frac{2r}{a}\right)$.

This coefficient differs very little from that adopted by M. Ramond, viz. 18336 m.: this he deduces in his first memoir; it $= 60158{\cdot}7$ feet, but the variable multiplier $\left(1 + \frac{2r}{a}\right)$ does not appear in his formula. If in Biot's we take a mean value of (r) at 400 m. since any value of r must be very small compared with a, and substituting for (a) its value, the fraction will continue very small, and we shall have $18334{\cdot}11 \times 1 + {\cdot}00012$ nearly, which gives 18336·3 for the constant coefficient.

II. The publication of M. Ramond from which I have given the foregoing abstract, comprises in the first place four memoirs of the highest interest discussing various points connected with the subject of barometric observations. These are followed by a second part, entitled, "Elementary and Practical Instructions for the Application of the Barometer to the Measurement of Heights." It is this part of the work which I have here abridged, and which may be considered as in some degree bringing together the results of experiments detailed in the preceding memoirs. Those relating to practical directions for observing appear to me sufficiently detailed in the "Instructions;" but one or two points connected with the formula, and discussed in the first memoir, may, I conceive, be here properly introduced to the more particular attention of the reader.

In his first Memoir, Part I. M. Ramond has given the results of barometrical measurements, which have shown him the necessity of augmenting the constant coefficient adopted by M. Laplace 17972·1 m. by rather less than 1-42d, so that it becomes 18393 m. or in feet 60345. He gives the measured height of four mountains, which he compares with the height computed by the several formulæ of Laplace (with the new coefficient), Trembley, Kirwan, Shuckburgh, and Roy (coefficient 184·4), the first being constantly found the most preferable. "As the ultimate result," he says (p. 11), "in eight observations, made with peculiar care, the formula of M. Laplace, with the new coefficient, has been correct five times, and that of Trembley only twice. Now in these eight observations the mean temperature varied from 8·375° to 19·53°, and we are in

consequence authorized to conclude, that the formula of M. Laplace keeps nearer the truth, and is less dependant on variation of temperature than the others."

He then proceeds to point out the divergence of the other formulæ from the truth according to variations of the temperature.

The second part of this memoir is devoted to an examination of the correction for the diminution of gravity corresponding to the latitude. The accuracy of the expression for this purpose given in the formula is shown by a comparison of M. Humboldt's observations with geometrical determinations of the heights of several mountains in Mexico and Peru.

The third part treats of the correction for the vertical diminution of gravity. An extensive comparison is made of the results of observations, employing in the first instance the exact formula comprising the correction in question, and in the second, dispensing with that correction by an augmentation of the constant coefficient from 18336 m. to 18393 m. Fourteen measurements are thus compared, and the results differ but little. At the end of the memoir an example is worked out by both methods. The difference is about nine feet in the height of Chimborazo.

In the fourth part M. Ramond examines the results deducible from the formulæ which he had before compared together, in relation to the ratio which they respectively give between the weights of air and mercury; comparing also this ratio with that given by experiment.

He commences by reducing each of the four formulæ of Laplace, Trembley, Kirwan, and Shuckborough, to a similar form, thus separating in each the corrections for the temperature both of air and mercury, and the constant coefficient, both when taken with the mean constant correction as examined in the preceding part, and when uncorrected.

	Laplace.	Trembley.	Kirwan.	Shuckborough.
Ord. coeff. t. 0° lat. 45	18393	18322·976	18287·83	18425·188
Coeff. reduced to level of sea	18336	18266·193	18231·156	18368·088
Factor for temperature of air	$\frac{1}{250}$	$\frac{1}{225{\cdot}625}$	$\frac{1}{222{\cdot}2222}$	$\frac{1}{254{\cdot}0625}$
Dilatation of mercury	$\frac{1}{5412}$	$\frac{1}{5400}$	$\frac{1}{6020}$	$\frac{1}{5400}$

From each of these the resulting ratios of the weights of air and mercury are as follows:

Pressure.	Temperature.	Laplace.	Trembley.
0·758 m.	12·5°	1 : 11030·85	1 : 11045·379
	17·5	1 : 11240·963	1 : 11277·30

Kirwan.	Shuckborough.
1 : 11033·06	1 : 11053·25
1 : 11268·08	1 : 11248·84

Brisson has determined the specific gravity of mercury at 17·5° to be 13·5681, that of water being 1.

The same philosopher has also given from the best experiments the weight of atmospheric air at 12·5°, and that of water at different temperatures.

A cubic decimetre of air at the pressure of 0·758 m. and at the temperature 12·5°, weighs, according to him	1·2319025 gr.
The same volume of water at 12·5° weighs in air	998·064125
Thence the weight of the water in vacuo =	999·2960275

On the other hand, a cubic decimetre of water at 18·75° weighs in air of the same temperature 997·445669 gr. Supposing the dilatation to be nearly uniform within the limits of temperature here considered, at 17·5°, the volume of water will weigh in air 997·569444 gr.

From these experiments it results on the one part that the pressure being 0·758, and temperature 12·5°, the weight of air is to that of water in vacuo as 1 : 811 . 1814. On the other hand, it results that mercury of the temperature 17·5° weighs 13535·12 gr.

Now these ratios being at 5°, difference in temperature cannot be compared without reference to what we know of the dilatations of mercury and air; namely, by reducing the mercury to 12·5°, or the air to 17·5°; but the authors of the four formulæ above analyzed do not agree in the law of these dilatations. The different results deduced from these experiments, according to the dilatations assumed by each author, are as follows:

Pressure.	Temperature.	Laplace.	Trembley.
0·758 m.	12·5°	1 : 11010·86	1 : 11010·883
	17·5	1 : 11214·128	1 : 11236·39

Kirwan.	Shuckborough.
1 : 11009·85	1 : 11010·883
1 : 11239·88	1 : 11210·765

He considers the results at 12·5° most deserving confidence, and that Kirwan's dilatations are too small.

These results compared with the former set show the degree of correspondence between experiment and deduction from the barometric formula. The formula of Laplace agrees most nearly with experiment; the difference admits of a satisfactory explanation if we only consider the different quantities of moisture held in solution by the air, under the very different circumstances of a confined room, and the top of a mountain; and this difference being only about $\frac{1}{552}$d part would only affect the accuracy of about 10 metres even in the height of Chimborazo; and after all, the ratios which the formula gives, being so many means deduced from a great number of observations, and so many conclusions deduced from operations on a large scale, and applied to those on a small, are more proper to give confirmation to the results of experiment than to receive it from them.

In a note appended to the beginning of the second memoir, M. Ramond quotes an account of a more recent determination of the ratio of the weights of air and mercury; which results 1 : 10463, the air being perfectly dry: in the latitude of Paris, temperature 0°, pressure 0·76 m. This result was obtained by MM. Arago and Biot. From it they deduce the barometric coefficient, for lat. 45 in metres, 18316·6 for dry air, and 18351·8 for air saturated with moisture; and for the mean state 18334·2, which is very nearly equal to that adopted by M. Ramond from observation confirmed by geometrical measurement, viz. 18336.

III. Under the head of "Isolated Observations," M. Ramond discusses the question of the decrease of temperature as we ascend in the atmosphere. He has given in the original, a table exhibiting this decrease from a variety of observations, the *result* of which examination only I have preserved in the foregoing abstract. The reader will find the supposition of an uniform decrease (which M. Ramond took as a mean value convenient for practical purposes), confirmed by reasoning *à priori* in the valuable paper on Barometrical Measurements, by Prof. Playfair, in the Edinb. Transactions, vol. i. 1788, and since republished in his works, vol. iii. 1822. In this memoir, Part III. the author investigates the law of decrease in the heat of the different strata of air as we ascend. He gives a demonstration, proving, that abstracting from certain anomalies annual and diurnal, as well as from accidental irregularities, the decrease is uniform. This proof is deduced upon the principle, that the sun's rays do not heat the air in their passage through it; a fact established by many concurrent experiments.

IV. In adverting to the necessity of reducing the mercury in the cistern of the barometer to a constant level, M. Ramond has mentioned several contrivances of distinguished foreign artists for this purpose. The accuracy, however, of all such expedients appears very questionable; and as a constant point of departure

in the scale is a very important and fundamental condition when any thing like precision is attempted, it may be proper, for the sake of such readers as may not have had much experience in these operations, to state the mode of making this correction by calculation; a method, which, it may safely be presumed, must be more correct than any mechanical contrivance.

First, we must suppose that we have given the internal diameter of the cistern (supposing it to be truly cylindrical), which we will call (D); secondly, the external diameter of the tube (d); and thirdly, its internal diameter (d^1). It is obvious that the increment or decrement of the height of the mercury in the tube (h_1) will be accompanied by a corresponding decrement or increment in the cistern (h^1); and this, in the inverse ratio of their areas. This ratio will be that of ($D^2 - d^2$) to (d'^2); which we will call $\left(\frac{A}{a}\right)$; and consequently $h^1 = h_1 \left(\frac{a}{A}\right)$.

The quantity (h_1) is here obviously supposed to be measured from some fixed point at which the scale of the instrument becomes accurately true, the mercury in the cistern being exactly at the zero of the scale. Supposing this point to be 30 inches, and (h) the observed height of the mercury, the correct height (H) will be $= h \pm h_1 \left(\frac{a}{A}\right)$, h_1 being + when above 30, and − when below. The ratio $\left(\frac{a}{A}\right)$ is to be determined once for all for the particular instrument we employ, and the whole operation at each observation is reduced to merely taking the difference of the observed height above or below 30 inches, or the standard point of the scale, multiplying that difference by the constant ratio, and adding to or subtracting from the observed height.

Example.—Suppose from measurement we found

$d^1 = \cdot 14$ inch. $\quad D = 1\cdot 21 \quad d = \cdot 4$

Then $d'^2 = \cdot 0196 \quad D^2 = 1\cdot 4641 \quad d^2 = \cdot 16$

Hence $D^2 - d^2 = 1\cdot 3041$ and $\frac{a}{A} = \frac{196}{13041} = \frac{1}{66}$ nearly.

$$\begin{aligned} \text{Suppose we observe } h &= 31\cdot 234 \\ \therefore h_1 &= 1\cdot 234 \\ \text{And } h_1 \times \tfrac{1}{66} &= 0\cdot 018 \\ \therefore \text{H} &= 31\cdot 252 \end{aligned}$$

The fraction thus obtained for each individual instrument is marked with a diamond on the tube near the top by the maker.

The measurements from which these fractions result may be depended upon to the 100th of an inch, as I have been informed by Mr. Cary. In his barometers, the point of no correction, if

it be any other than 30 inches, is distinguished by a mark on the scale.*

V. In forming the preceding compendium, one considerable source of abridgment was found in omitting altogether many details on the subject of the *hygrometer*. This I have been induced to do from two reasons; first, the methods described by M. Ramond apply solely to the use of hygrometers on the old constructions, and are both long, and probably inaccurate, when compared with the more improved methods now generally adopted on the principle of evaporation. Secondly, M. Ramond himself only treats of them as connected with the *stationary* meteorological observations. He conceives them of little use in the measurement of heights as the following quotations will clearly show:—

(Second Memoir, § 3, p. 57.)—Allowing all that can reasonably be done to the error of the instrument, it is still certain that I have made observations at extremely different degrees of humidity; and that nevertheless the effect of this circumstance has been covered in extraordinary cases, by that of more preponderating circumstances by which they were accompanied; and in ordinary cases by even the allowance due to the error of observation. The reason is evident: the factor for the temperature having been empirically determined contains the correction for the mean humidity; and the quantities by which this mean humidity has to be augmented or diminished, are ordinarily too small to affect sensibly results, on which the least accidents produce a greater effect than these quantities.

(Third Memoir, Part III. p. 99.)—It is well known that the mixture of vapour diminishes the weight of air; but we know also the limits within which this action is confined; and if we admit it into the number of causes which determine the variations of the barometer, we are not ignorant that it is far from completely accounting for them. Even when atmospheric air is susceptible of passing naturally to the state of dryness to which we can bring it artificially, the return from this state to that of saturation will only diminish the elevation of the column of mercury, by from a sixtieth to a fiftieth part, according to the temperature of the mixture; but experience proves that the air never approaches to absolute dryness, and that it always retains a considerable dose of moisture, so that the usual variations in this respect will scarcely account for a variation of a 120th or a 100th part. Now the oscillations of the barometer in our climate run through a space equal to at least 1-18th of the total height; and the barometer rises and falls frequently in the oppo-

* Should the preceding remarks, or any others in this appendix, appear of a more elementary nature than are usually the topics of discussion in scientific journals, the author begs to state, that the whole was originally drawn up with a view to separate publication; and he conceived he was consulting the convenience of many readers in giving the detail and reasons of every part of the operations.

site order to the augmentations and diminutions of humidity. We conclude, therefore, that the effects of this cause are counteracted by those of a cause so preponderating, that, after having compensated the action of humidity, the excess of its own influence extends yet further.

(Instructions, p. 197.)—The hygrometer has not yet been of any utility in the mensuration of heights, and there is little probability that it can be introduced, not only because the correction will be very small, but further because it will be very uncertain, whether we consider the ignorance in which we are of the law which the decrease of humidity in the column of air follows; or the extreme difficulty, if not impossibility, of eliminating this law in the result of experiments always made at the surface of the earth; that is to say, at the very source of those influences which modify partially and irregularly the humidity of the atmosphere. Saussure thought thus, and we are of the same opinion. The mean value of the humidity comprised in the constant coefficient and the factor belonging to the temperature, will occasion less error than a theory ill supported by observations will do; and these errors after all are of such small consequence, that they are not worth the trouble of a calculation, which will only cause a variation in the chances, even if it do not multiply them.

Such are the opinions of M. Ramond on the subject of a correction for the different state of moisture in which the air may be at the two stations; and to his great practical experience we must doubtless pay the highest deference; at the same time it becomes necessary to recollect the great improvements which have taken place in the science of hygrometry subsequently to the date of the methods described and used by our distinguished author. Hence several philosophers of the present day have not considered it undeserving attention to examine into the propriety of introducing the correction in question. Some observations seem to indicate an effect by no means inconsiderate due to the presence of vapour. We may cite the instances of Mr. Greatorex's observations on Skiddaw (Phil. Trans. 1818, Part II.), in which a considerable discrepancy appears to have been connected with some changes in the hygrometric state of the air. The measurements of Messrs. Herschel and Babbage at Staubbach (Edinb. Phil. Journ. No. 12), seem also to have been affected by the same cause. I merely refer to these cases, however, in order to observe in general that should more extended observations show the necessity of an application of the hygrometer in barometrical operations, the formula above investigated will easily admit of the introduction of a variable factor for this correction, instead of the mean value at present involved in the constant coefficient, and slightly modified by the variation of temperature.

An excellent method of ascertaining the elastic force of the

vapour actually suspended in the atmosphere at the time of observation is given in the Edinburgh Encyclopedia, Art. Hygrometry, attributed to Mr. Anderson.* The hygrometer employed may consist merely of two common thermometers; one is essentially necessary to the observer for taking the temperature of the air; and the other is to be compared with it, having its bulb covered with moistened linen; and will but little increase the apparatus.

The details are not of difficult investigation; but as experience has not yet decided on the propriety of introducing the correction, I shall not at present proceed to any further particulars.

Article VIII.

On Titanium. By M. H. Rose.†

The oxide of titanium used in these experiments was procured from the rutile of Saint-Yrieix department de la Haute-Vienne.

When this oxide is fused with carbonate of potash, it forms a compound which sometimes becomes gelatinous when muriatic acid is added; but it is never as thick as that formed by silica. Oxide of titanium which has been heated to redness, when moistened and put upon litmus paper, becomes red without affecting the colour of the paper. The effect of this oxide upon litmus is more distinctly shown by putting a small quantity reduced to powder upon a drop of the tincture placed upon a white surface. The oxide becomes red as soon as it is touched by the tincture.

Oxide of titanium forms compounds with the alkalies in which it acts as an acid. It is true that it also combines with acids, forming insoluble compounds which do not possess the properties of salts, but rather of double acids. For these reasons, M. Rose considers the oxide of titanium as an acid, and distinguishes it by the term titanic acid; but states that, like columbic acid and silica (which is considered as an acid by M. Rose), its affinities at common temperatures are extremely weak, on which account it is difficult to ascertain its properties, and especially to determine its saturating power, and the quantity of oxygen which it contains.

The author then states that he used three modes to ascertain its saturating power; first, by examining its combinations with the alkalies; secondly, those insoluble compounds it forms with some acids; and lastly, by combining it with sulphur; and the

* See also the Edinb. Phil. Journ. No. 4, p. 369.

† Extracted from the Annales de Chimie et de Physique, t. xxiii. p. 353.

analysis of the sulphuret of titanium was the only method which afforded satisfactory results.

Acidulous titanate of soda appeared to be composed of

Exper. 1.	Titanic acid.	83·15
	Soda.	16·85
		100·00
Exper. 2.	Titanic acid.	83·14
	Soda.	16·86
		100·00

When this acidulous titanate of soda is treated with muriatic acid, a part of the soda is taken from it, and a compound formed which gave:

Exper. 1.	Titanic acid.	96·20
	Soda.	3·80
		100·00
Exper. 2.	Titanic acid	96·56
	Soda.	3·44
		100·00

The experiments performed upon the acidulous titanate of potash did not correspond with the results obtained with acidulous titanate of soda; this compound which had been heated to redness was composed of

Exper. 1.	Titanic acid	81·99
	Potash	18·01
		100·00
Exper. 2.	Titanic acid.	82·67
	Potash	17·33
		100·00

Titanate of potash containing still more acid was composed of

Titanic acid	91·30
Potash	8·70
	100·00

As these analyses did not lead to satisfactory results, M. Rose tried the method of determining the point of saturation by the quantity of carbonic acid, which the titanic acid was capable of expelling from carbonate of potash at a red heat.

In order to ascertain whether this method might be relied

upon, substances containing a known quantity of oxygen were submitted to experiment. A mixture of silica and carbonate of potash was heated together in a small platina crucible; and it appeared by every trial, that the quantity of oxygen in the carbonic acid expelled was equal to that in the silica employed, as indicated by the experiments of Berzelius, the mean of result being 50·3 and the mean of M. Rose's 50·27.

M. Rose states the results of five experiments, in which carbonate of soda and titanic acid were heated together. He has not reduced them to centesimal parts, but by doing so it will appear that he was less fortunate than with the experiments on silica. Supposing 100 parts of the titanic acid to have been used, the quantity of oxygen of the carbonic acid expelled by it, and consequently that of the titanic acid itself, would have amounted in

Exper. 1	33·639 per cent.
2	37·019
3	36·034
4	35·024
5	33·534

It is evident that this method did not succeed; the compound obtained by heating titanic acid with carbonate of soda, M. Rose considers as a neutral titanate, which is decomposed by water, it taking away part of the potash.

Compounds of Titanic Acid with Acids.

All chemists who have made experiments upon titanium have admitted the existence of salts, in which the oxide of titanium is considered as a base. According to them the sulphates, nitrates, and muriate, crystallize after evaporation. M. Rose supposes, however, that what has been considered pure oxide of titanium, is a compound of titanic acid with the alkalies, and he imagines their titanic salts are combinations of the alkalies with the acids employed. In fact, when acidulous titanate of potash is dissolved in muriatic acid, cubic crystals of chloride of potassium are obtained; and the author is of opinion that no compounds of titanium exist, in which the titanium can be considered as the base. When indeed acidulous titanate of potash is dissolved in muriatic acid, some acids throw down precipitates which contain no potash, and do not possess the properties of salts. These precipitates redden litmus paper strongly, and they must be considered as insoluble double acids, analogous to the compounds of tartaric acid with some other acids, which have been described by Berzelius.

When acidulous titanate of potash is dissolved in muriatic acid, and the solution is diluted with water, white precipitates are obtained by adding the sulphuric, arsenic, phosphoric, oxalic, and tartaric acids, but no precipitate is formed by the addition

of nitric, acetic, or succinic acids. All these precipitates are soluble, not only in an excess of the acid employed, but also in that of the titanic solution. The filtered solutions contain potash.

Compound of Titanic and Sulphuric Acids.

When this compound is heated to redness, the sulphuric acid and water of crystallization are expelled, and the titanic acid remains pure. It is extremely deliquescent, which renders the attempts at analysis mere approximations. It reddens moistened litmus paper strongly. To analyze this compound, it was dissolved in muriatic acid, the titanic acid was precipitated by ammonia, and the sulphuric by muriate of barytes. Two experiments gave the following results:

Titanic acid	76·83	76·50
Sulphuric acid	7·78	7·56
Water	15·39	15·94
	100·00	100·00

Combinations of Titanic Acid, with the Arsenic and Phosphoric Acids.

These have the appearance of alumina; when dried, they have the lustre of gum arabic.

Titanic and Oxalic Acid.

Two experiments gave the following as the composition of this double acid.

Titanic acid	74·42	73·77
Oxalic acid	10·25	10·56
Water	15·33	15·67
	100·00	100·00

Titanic and Tartaric Acids.

This compound resembles the preceding; but no analysis of it is given. When heated in contact with air, it becomes with some difficulty white; and when heated without the presence of air, a black powder resembling carburet of titanium is obtained, but its nature was not determined.

Titanic Acid and Silica.

When pure titanic acid and excess of silica are fused with carbonate of potash, and the fused mass is treated with water, the excess of silica is dissolved by the potash, and an insoluble compound of titanic acid, silica, and potash, is obtained. This compound may be arranged with the salts which are composed of one base and two acids, few of which are obtainable arti-

ficially, but which are formed by nature, as in the datholite and botryolite. This compound is readily soluble at common temperatures in muriatic acid; it is analogous to sphène, which is composed of titanic acid, silica, and lime.

Experiments to reduce Titanic Acid, and to combine Titanium with Sulphur.

MM. Hecht, Vauquelin, and Laugier, tried to reduce titanic acid by charcoal; they obtained principally carburet of titanium with a small quantity of metallic titanium; which indeed was scarcely proved. Having found this product to be insoluble in acids, and even in aqua regia, they could not determine the quantity of oxygen contained in titanic acid. The carburet of titanium even if burnt in oxygen gas, would not have given correct results, this product being probably mixed with much titanic acid.

MM. Faraday and Stodart, in their experiments upon steel, have in vain attempted to alloy iron with titanium, and Vauquelin and Hecht had previously failed. M. Rose could not succeed in alloying zinc with titanium. Sulphuretted hydrogen did not at all act upon titanic acid; a black powder was formed, but no sulphuret of titanium, nor was a sulphuret produced by fusing sulphuret of potassium with the titanic acid.

M. Rose at last succeeded in forming the sulphuret by passing sulphuret of carbon over titanic acid strongly heated in a porcelain tube. This sulphuret is of a deep green colour; when rubbed with a hard body, it assumes a very strong metallic lustre resembling brass. When heated in contact with atmospheric air, it burns with a sulphurous flame, and is converted into titanic acid. When heated in a small narrow-necked retort, a small quantity of sulphur is separated if the aperture be closed, but not otherwise. It becomes very hot when nitric acid is poured upon it; nitrous vapours are emitted, the fluid becomes milky, and titanic acid is deposited in the state of a fine powder; when the fluid is boiled, the sulphur melts, and aggregates into small masses.

This sulphuret of titanium was analysed by combustion upon platina by means of a spirit lamp; 1·017 of pure and solid sulphuret of titanium gave 0·767 of pure titanic acid.

Calculating upon the results of this experiment, M. Rose concludes that titanic acid is composed of

Titanium	66·05
Oxygen	33·95
	100·00

And the sulphuret of

Titanium	49·17
Sulphur	50·83
	100·00

Article IX.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 288.)

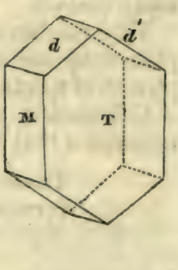
Having dissolved and recrystallised several of the salts described in these communications, I have observed differences in the figures of what may be termed different *crops* of crystals obtained from the same solution. Having dissolved some chromate of soda, the crystals first deposited, or *first crop*, as we may term them, were all lengthened in the direction of the great diagonal of their terminal planes, so as to be almost acicular. These crystals having been taken out of the solution, a second crop was soon deposited, many of which nearly agreed in form with the engraved figure already given, but most of them were much flattened or reduced in height, so as to become what has been termed tabular, and apparently bearing no relation to the slender crystals first produced.

The same difference of character is found to obtain in many other salts. When these varieties of figure occur, the goniometer will afford sufficient evidence that their differences are only apparent, and that they are really analogous forms whose character has been varied by a disproportionate extension of some of the planes of the crystals in particular directions.

Acetate of Lead.

I have received some brilliant crystals of this substance from Mr. R. Phillips, several of which have given measurements on the corresponding natural planes agreeing within 3′ or 4′, and affording an example of unusual regularity of form.

The crystals may be cleaved parallel to the lateral and terminal planes, of a *right oblique angled prism*, which may be regarded as its primary form. The only modification I have observed is exhibited in the annexed figure.

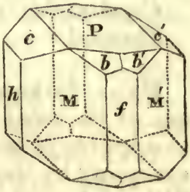


d on *d*′	128°	0′
d on M	116	0
d on T	98	30
M on T	109	32

Oxalate of Ammonia.

I have not observed any distinct cleavage of the crystals of this salt, but their forms are referable to *a right rhombic prism* as the primary. They are subject, however, to an irregularity of

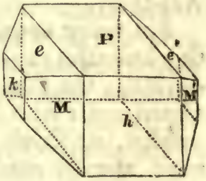
figure, analogous to some which have been before noticed; there being on some of the crystals only one of the planes *b* replacing each of the solid angles on which two are placed in the drawing, and these being the alternate planes. Many of the crystals present, however, the pairs of planes *b*, as shown in the figure.



P on M, or M′	90°	0′
P on *c*, or *c*′	143	30
c on *c*′	107	0
c on *h*.	126	30
M on M′	104	6
M on *f*	142	3
M on *h*	127	57
M on *b*	121	0
M′ on *b*.	97	21

Carbonate of Magnesia.

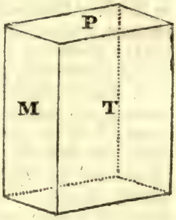
The crystals from which this figure has been given, I have received from M. Teschemacher. The primary form is *an oblique rhombic prism*, which may be cleaved, but not distinctly in the small crystals I have attempted to operate upon, parallel to the planes M and M′.



P on M, or M′	102°	0′
P on *e*, or *e*′	120	30
M on M′	86	30
M on *h*.	133	15
M on *k*	136	45

Sulphate of Cinchonia.

Mr. Pope, of Oxford-street, has favoured me with some minute crystals of this salt: from which the primary form appears to be *a doubly oblique prism*, having cleavages parallel to all its planes. The cleavage, however, parallel to P is not very distinct. Some of the crystals are of the form I have given, but there are others whose figure does not appear to be immediately related to it. These are probably hemitrope, or rather quadruple crystals, united by secondary planes; but they are not sufficiently distinct in character to enable me at present to trace their precise relations to the primary form.



P on M	95°	50′
P on T.	90	0
M on T	83	30

Article X.

On the Property which some Metals possess of facilitating the Combination of Elastic Fluids. By MM. Dulong and Thenard.*

Prof. Döbereiner, of the University of Jena, has discovered one of the most curious phenomena which physical science is susceptible of unfolding. We are unacquainted with his labours excepting by the announcement in the Journal des Debats of the 24th of August last, and which does not give a very correct account of them, and from a letter of M. Kastner to Dr. Liebeg, which the latter, now at Paris, has communicated to us. In this it is stated, that platina† in a spongy mass occasions the combination of oxygen and hydrogen at common temperatures, and that the extrication of heat which results from the action renders the metal red-hot. We were anxious to verify so surprising a fact, and found it perfectly correct. As the experiment may be made with the greatest ease, we shall perform it before the Academy.

Not being acquainted with the researches which the author of this beautiful experiment has undoubtedly undertaken in order to develope the theory of it, we could not refrain from making some experiments with this view; and although we have not yet succeeded, we think that the results of the observations which we have already made are not unworthy the attention of the Academy.‡

In the experiment which we have made, the spongy platina became red-hot when placed where the hydrogen escaping from the reservoir became intimately mixed with the air. It was evident from this, that detonation would occur by immersing a piece of the spongy platina in a mixture of two volumes of hydrogen and one volume of oxygen; and this was confirmed

* Annales de Chimie et de Physique, tome xxiii. p. 440.

† Since the printing of this notice, the authors have observed first, that palladium in a spongy mass is capable of inflaming hydrogen as platina does; secondly, that iridium in the same form became very hot, and produced water; thirdly, that cobalt and nickel in mass, at about 572° Fahr. effected the union of hydrogen and oxygen; fourthly, that spongy platina at the common temperatures formed water and ammonia with nitrous gas and hydrogen, and acted upon a mixture of hydrogen and protoxide of azote.

‡ The hydrogen lamp invented by M. Gay-Lussac is extremely convenient for performing this experiment. The electrophorus is to be removed, or the conductors are merely to be detached; a piece of light spongy platina is to be placed at the distance of about two centimetres from the aperture at which the gas escapes; when the cock is turned, the jet of hydrogen gas comes mixed with atmospheric air to the surface of the spongy platina. It then soon became red-hot, and the hydrogen gas once inflamed continues to burn as it escapes, as if it had been lighted by the spark.

In the absence of the lamp, the common apparatus used for preparing hydrogen gas may be employed. It is however requisite to take care that the gas passes through a very small aperture, in order that it may more intimately mix with atmospheric air.

by experiment. If the proportions of the gaseous mixture differ much from those which form water, or if an incombustible gas, such as azote, be present, the combination goes on slowly, the temperature is but little increased, and water soon condenses in the receiver.

If the spongy platina be strongly calcined, it loses the property of becoming red hot; but in this case, it effects the combination of the two gases slowly, and without any very sensible increase of temperature. Platina reduced to a very fine powder, by well-known chemical means, does not act upon the gases even slowly, at common temperatures, nor do platina wires or bars. The agreement of these observations may give rise to the idea, that the porosity of the metal is an essential condition in the production of the phenomenon; but the following facts disprove this conjecture.

We reduced platina to leaves as thin as the malleability of the metal would allow of. In this state, the platina acts at common temperatures upon the mixture of hydrogen and oxygen, and the action is more rapid when the foil is thinnest. We obtained some which effected the detonation in a few seconds. But what renders this action still more extraordinary is the physical condition indispensable to its production. A very thin sheet of platina rolled on a glass cylinder, or freely suspended in a detonating mixture, produced no sensible effect after a lapse of several days. The same sheet of platina, if crumpled, acts instantaneously, and causes the mixture to detonate.

The leaves disposed as we have described, and which produce no effect at common temperatures, the wires, powder, and thick bars of platina, which are inefficient under the same circumstances, act slowly, and without producing explosion at a temperature of 400° to 572° according to their thickness.

We have found that other metals possess the same property as platina. The very remarkable fact discovered by Sir H. Davy during his researches on the safety lamp, viz. that wires of platina and palladium at a low temperature become bright-red when immersed in a detonating mixture, having appeared to us to be derived from the same cause as the phenomenon under discussion, we were first induced to try palladium.

The piece which we made use of was given to one of us by Dr. Wollaston, and consequently must be considered as free from alloy; nevertheless we were unable to reduce it to very thin leaves, as it cracked under the hammer. We attribute to this circumstance its possessing no action at the temperature of the atmosphere; but it acted at least as well as platina of the same thickness at a high temperature. Rhodium being brittle could not be subjected to the same preparation; but it occasioned the formation of water at a temperature of about 464° of Fahr.

Gold and silver in thin leaves act only at high temperatures, but always below that of boiling mercury. Silver is less power-

ful than gold. A bar of gold acts, but with greater difficulty than the leaves; a thick bar of silver acts so feebly as to be questionable whether it has any power.

We have examined whether other combinations could be effected by the same method. Oxide of carbon and oxygen combine, and nitrous gas is decomposed by hydrogen at common temperatures by spongy platina; thin sheets of this metal require a temperature of above 572° of Fahr. to cause the two former gases to combine. Gold leaves effect it also at a temperature approaching that of boiling mercury.

Lastly, olefiant gas mixed with a proper quantity of oxygen is completely converted into water and carbonic acid by spongy platina, but only at a temperature above 572° of Fahr. It will be remembered on the subject of the preceding experiments, that one of us proved a long time since, that iron, copper, gold, silver, and platina, possess the property of decomposing ammonia at a certain temperature, without absorbing any of this alkali, and that this property appeared to be inexhaustible. Iron possesses it in a greater degree than copper, and copper more than silver, gold, or platina, the surfaces of all being equal.

One hundred and fifty-four grains of iron wire were sufficient to decompose within a few hundredths a current of ammoniacal gas rather rapidly evolved, and continued during eight to ten hours, without the temperature exceeding the limit at which ammonia completely resists. Three times the quantity of platina wire of the same size scarcely produced an equal effect, even at a higher temperature.

The remarkable results of this experiment depend perhaps upon the same causes as those which occasion gold and silver to effect the combination of hydrogen and oxygen at 572° Fahr.; platina in mass at 518° Fahr.; and spongy platina at common temperatures. If then we observe that iron, which so readily decomposes ammonia, does not effect, or effects with difficulty, the combination of hydrogen with oxygen, and that platina, which is so powerful in the latter case, scarcely decomposes ammonia, we are induced to suppose that some gases have a tendency to combine under the influence of the metals, and others to separate; this property varying on account of the nature of each. Those metals which produce one of the effects most perfectly are incapable of producing the other, or in a less degree.

We shall refrain from offering the conjectures which these singular phenomena have given rise to, until we have completed the experiments which we have undertaken to verify them.*

* Prof. Döbereiner's experiment has also been verified by Mr. Faraday, who has given the following notice of it in No. 31, of the Journal of Science. "It consists in passing a stream of hydrogen against the finely divided platina, obtained by heating the muriate of ammonia and platina. In consequence of the contact, the hydrogen inflames. Even when the hydrogen does not inflame, it ignites the platina in places; and I find that when the hydrogen is passed over the platinum in a tube, no air being admitted, still the platinum heats in the same manner."

Article XI.

Notice of some newly discovered Islands in the Arctic Sea. By Capt. Duncan: communicated in a Letter from L. Edmonston, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Zetland, Balta Sound, Sept.* 12, 1823.

The public attention has been recently so much directed to Arctic discoveries, that I flatter myself the following communication may be acceptable to your journal.

The Greenland ship Dundee, of London, arrived here on the 10th inst.; and her very enterprising Commander, Capt. Duncan, obligingly furnished me with the following information which is contained almost verbatim in his diary. "Sept. 2, in lat. about 68° 40′; long. 24° 30′ W.; foggy weather and east winds (latter part of the day clearer) blowing very fresh. Ship running in north-west towards the land; at 9, a. m. got within two miles of a small island bearing north-west, which I named Sayers Island, after the master of the Harmony, of Hull, then in company; the mainland running about NNE and SSW, distant about fourteen miles. The nearest headland on it in right bearing north, I named Cape Despair, distance six leagues. Cape Barclay of Scoresby's Chart, bore north-east and east, distance 50 miles; and the most southern headland on the main bore west and by south, distance 60 miles: this I named Duncansby Head. All the mainland seen from the ship between this point and Cape Barclay, I named Gales Land, in compliment to my owner. About 10 miles south-east from Duncansby Head, there is a low flat island which I termed Robison's Island, after the ship's managing agent. Here we lay to, hoping to see fish, but fell in with none; and the sea setting in heavy towards the land, and the wind blowing fresh, we stood off to the south.

"At noon latitude observed 68° 41′; long. 24° 30′ W; by the bearings of Cape Barclay; sounded in 100 fathoms water; rocky bottom. Saw all this new land for twenty-four hours; the Harmony, of Hull, in company all the time; but the gale and sea prevented any attempts at landing. Had intended prosecuting investigation further southwards, but the lateness of the season, and the unfortunate accident of being *beset* nearly two months this summer, made all thoughts of such a view imprudent."

Gales Land, Capt. Duncan states, resembles in general appearance the south side of Scoresby's Sound. It is very high, and precipitous quite to the sea shore. The mountains running in ridges south-east and north-west, but their peaks are not so

prominent or conical as in Scoresby's Sound. The north sides of the mountains were snowy; the south, green. With the exception of a very deep inlet south from Robison's Island, the coast was little indented.

Capt. D. was at one time within six or seven miles of the mainland, about forty miles north from Robison's Island, which was considerably verdant, very flat, and apparently about ten miles long, and five broad.

Sayers Island is rocky and barren, about half a mile long, and one-quarter broad.

There was little fast and not much drift ice to be met with. A good deal of drift timber was observed floating, and several icebergs grounded along the shore. The current was setting without interruption during the twenty-four hours that the two vessels were in that quarter, south and west, at the rate of one and a half mile per hour. There was no inset or offset of the tides observed. No whales were seen, and few seals, or birds, except kittiwakes; these were abundant. No appearance of natives. The weather was very sleety.

Gales Land, therefore, seems to form the imaginary line of coast laid down in Scoresby's Chart, published in his recent "Journal of Discoveries in the Arctic Regions," extending from Cape Barclay in the north to Ollumlongni Frith on the south; and the island laid down there north of this frith would seem to be what Capt. Duncan has termed Robison's Island: it lies in about 67° lat.; 25° long. He was at one time of the day within five miles of it. In the years 1821 and 1822, he had coasted almost all the land described by Scoresby north of Cape Barclay; and was as far as 40 miles up Scoresby's Sound, and he bears testimony to the accuracy of that intelligent navigator.

Jameson's Land he believes to be an island.

This voyage, which reflects so much credit on the enterprise and skill of Capt. Duncan, promises to be highly interesting to arctic geography; and may throw light on the fate of the lost colonies of Greenland; for it is highly probable that in Gales Land rather than any where else, they may be sought for with some chance of success. From Capt. Duncan's description, neither the climate nor the land seems to be inhospitable, or inaccessible if visited at a favourable period of the year.

I am, Sir, your obedient servant,

LAWRENCE EDMONSTON.

Article XII.

Analyses of Books.

Transactions of the Linnean Society of London. Vol. XIV. Part I. 1823.

(*Concluded from* p. 306.)

VII. *Account of the Lansium and some other Genera of Malayan Plants.* By William Jack, MD. Communicated by Henry Thomas Colebrooke, Esq. FRS. and LS.

This paper commences with the following observations:—

"There are a variety of highly esteemed fruits, which may be considered as peculiar to the Malayan Archipelago, or what has been not unaptly denominated *India aquosa*, and are not to be found beyond its limits. Many of these are already well known; but there are others which have not yet fallen under the observation of botanists, or are only to be found described in the *Hortus Amboinensis* of Rumphius, which, though a work of wonderful accuracy and research, stands in need of illustration with reference to the progress that has been made in botanical science since the period at which it was written. Among these the *Lanseh*, the *Tampooi*, and the *Choopa*, hold no undistinguished place, and the following account of these plants will therefore not be uninteresting. The first is already partially known from Rumphius, and Mr. Marsden's *History of Sumatra;* but its true place and family have hitherto remained doubtful. To these I have subjoined descriptions of a few other genera from the same interesting quarter, which appear to be new and to deserve notice."

LANSIUM.

Decandria Monogynia. N. O. Meliaceæ Juss.

Calyx 5-partitus. *Corolla* 5-petala, petalis subrotundis. *Tubus staminiferus* globosus, ore subintegro, antheris decem inclusis. *Ovarium* 5-loculare, loculis 1—2-sporis. *Stylus* brevis, columnaris. *Stigma* planum, 5-radiatum. *Bacca* corticata, 5-locularis, 5-sperma, uno alterove loculo tantum semen perficiente. *Semina* integumento exteriore pulposo sapido. *Albumen nullum ; cotyledonibus inæqualibus peltatis.*

Arbores, foliis pinnatis, floribus racemosis.

Lansium domesticum.

Langsat or Lanséh. *Malay.* Lansium. *Rumph. Amb.* i. *p.* 151. *t.* 54. Marsden's *History of Sumatra, pl.* v. *p.* 101. Native of the Malay Islands.

Var. β. *L. aqueum.*

Foliolis subtus villosis, racemis densis sæpius solitariis, fructibus globosis. Ayer Ayer. *Malay.*

"The Ayer Ayer so nearly resembles the Lanséh in most particulars, that I hesitate to rank it as a distinct species, and content myself with mentioning it as a permanent and well-marked variety. They are principally distinguished by the Malays by their fruit, that of the Ayer Ayer being rounder, and the pulp more watery (whence the name), and dissolving more completely in the mouth than that of the Lanséh. Both are highly esteemed by the Malays, and are equally agreeable to the European palate. The juicy envelope of the seeds is the part eaten, and the taste is cooling and pleasant.

"This genus has hitherto been known only from Rumphius's figure and description, and its place in the system has therefore continued uncertain. From an examination of the fruit, M. Correa de Serra conjectured it to be intermediate between the families of *Aurantiæ* and *Guttiferæ,* but the structure of the flower determines its true place to be among the *Meliaceæ.*

"I have further met in the forests near Bencoolen with a tree which appears to agree very nearly with the *Lansium montanum Rumph. Amb.* i. *p.* 154. *t.* 56. It differs in the number of the stamens, styles and seeds from the *Lansium* described above, but agrees with it exactly in carpological structure, and in general habit. Its characters coincide very nearly with those of Roxburgh's *Milnea.* They are as follow:

"*Calyx* five-parted. *Corolla* five-petalled. *Stamineous tube* subglobose, entire at the mouth ; anthers five, within the tube. *Styles* two. *Stigmas* two, simple. *Berries* globose, about the size of the domestic Lanséh, 1—2-celled, 1—2-seeded. *Seeds* enveloped in a thin subtransparent pulpy tunic or envelope, which has somewhat the flavour of the Lanséh, but with a bitterish and rather disagreeable smell."

"*Milnea* is perhaps scarcely distinct from *Lansium;* but if admitted as a separate genus, the above will constitute a second species, differing from *M. edulis* Roxb. in being digynous, and may be denominated *M. montana.*"

HEDYCARPUS.

Tetrandria Monogynia.

Perianthium 4-partitum, inferum. *Stamina* 4. *Ovarium* 3-loculare, loculis disporis. *Stigmata* tria. *Capsula* baccata, 3-valvis, 3-locularis, seminibus arillo sapido tunicatis. *Embryo inversus, albumine inclusus. Arbor foliis alternis simplicibus, floribus racemosis.*

The stamens are occasionally five in number, with a five-parted perianth and four-celled ovary.

HEDYCARPUS MALAYANUS.

Bera Tampui. *Malay.* Sumatra.

PIERARDIA. *Roxb.*

Perianthium 4-partitum. *Stamina* octo, brevia. *Ovarium* 3-loculare, loculis disporis. *Stigma* trifidum. *Bacca* corticata, trilocularis, loculis 1—2-spermis. *Semina* arillo sapido tunicata. *Embryo* inversus albumine inclusus.

Arbores, floribus racemosis, foliis alternis simplicibus.

PIERARDIA DULCIS.

Monoica, foliis obovatis. Bua Choopa. *Malay.* Sumatra.

"This species differs from that described by Roxburgh in being monoecious, in the form of the leaves, and in the colour of the fleshy aril. The Rambeh, of which Mr. Marsden has given a figure in his *History of Sumatra, pl.* vi. *p.* 101, so nearly resembles this, that I think it can only be a variety of the same. The Rambeh belongs to the peninsula of Malacca, and is unknown at Bencoolen; while the Choopa, which is abundant at the latter place, is not found in the former. The racemes of the Rambeh are longer and the fruit smaller than in the Choopa; but a comparison and examination of the two would be necessary to ascertain whether there are any essential differences, and I have not had an opportunity of doing this."

LEUCONOTIS.

Tetrandria Monogynia. N. O. Apocineæ. Br.

Calyx inferus, 4-partitus. *Corolla* tubulosa, superne angustior, limbo 4-lobo. *Stamina* 4, inclusa, laciniis corolla alterna. *Ovarium* simplex, biloculare, loculis disporis. *Stylus* 1, brevis. *Stigma* annulatum, apice conico. *Bacca* 1—3-sperma. *Semina* exalbuminosa, embryone inverso.

Frutex lactescens, foliis oppositis exstipularibus, floribus dichotome corymbosis axillaribus.

LEUCONOTIS ANCEPS.

Akar Morai. *Malay.* Sumatra.

"This singular plant belongs without doubt to the family of the *Apocyneæ*, with which its general appearance and habit entirely correspond. It agrees with *Cerbera* in having exalbuminous seeds; but its ovary is simple like that of *Carissa;* it will therefore hold an intermediate place between these two genera."

MYRMECODIA.

Tetrandia Monogynia. N. O. Rubiaceæ.

Calyx subinteger. *Corolla* quadrifida tubo intus ad insertionem staminum piloso. *Stamina* quatuor, corollâ breviora.

Stylus staminibus longior. *Stigma* simplex. *Bacca* ovata, quadrilocularis, tetrasperma.

Parasitica basi tuberosa, flores basibus petiolorum semitecti.

Myrmecodia tuberosa.

Nidus germinans formicarum rubrarum. *Rumph. Amb.* vi. p. 119. t. 55. *fig.* 2. Found at Pulo Nias.

"This singular plant is found parasitic upon old trees, in the form of a large irregular tuber, from which arise a few thick, short, fleshy branches. *The Leaves* are crowded at the rounded extremities of these branches, and are opposite, petiolate, obovate-oblong, with a short acumen, attenuated to the petiole, entire, very smooth, somewhat leathery. *Petioles* long, roundish, inserted on a large persistent peltate knob, whose edges expand into a kind of stipule, ciliated along the margin with dense strigose fibres, and cleft above in the axil of the petiole. The *flowers* are sessile, closely disposed in the spaces between the stipular bases of the petioles and half concealed under their projecting edges. *Calyx* membranaceous, superior, nearly entire. *Corolla* white, tubular, quadrifid; segments erect, rather acute; a villous ring within the tube immediately below the insertion of the stamens. *Stamens* four, shorter than the corolla, and alternate with its segments; *anthers* white, two-celled. *Style* longer than the stamens. *Stigma* simple, tomentose. *Ovary* four-celled, four-seeded. *Berry* ovate, smooth, white with longitudinal lines, four-celled, four-seeded. *Seeds* furnished with albumen; *embryo* in its axis.

"There can be no doubt of this being the plant described by Rumphius, although the leaves are represented more acute in his figure than they are in my specimens."

HYDNOPHYTUM.

Tetrandria Monogynia. *N. O.* *Rubiaceæ.* Juss.

Calyx integer. *Corolla* limbo 4-fido, fauce pilosâ. *Stamina* 4, brevia, fauci inserta. *Stigma* bifidum. *Bacca* disperma.

Super arbores parasitica, basi tuberosa, floribus axillaribus.

Hydnophytum formicarum.

Nidus germinans formicarum nigrarum. *Rumph. Amb.* vi. p. 119. *t.* 55. *fig.* 1. Prio Hantu. *Malay.* On trees in the forests of Sumatra.

"This grows parasitic on trees in the form of a large irregular tuber, fastening itself to them by fibrous roots, and throwing out several branches above. The tuber is generally inhabited by ants, and hollowed by them into numerous winding passages, which frequently extend a good way along the branches also, giving them the appearance of being fistular. *Leaves* opposite,

short-petioled, elliptic-obovate, nearly obtuse, acute at the base, very entire, very smooth, thick, with the midrib flattened, and a few inconspicuous nerves. *Stipules* interpetiolar, linear. *Flowers* axillary, sessile, generally aggregated on a double gemmaceous knob. *Calyx* superior, very short, entire. *Corolla*, white, tubular; *limb* four-cleft; *faux* villous. *Stamens* alternate with the segments of the corolla; *filaments* scarce any. *Ovary* crowned with a prominent umbilicate disk, disporous. *Style* longer than the tube. *Stigma* of two revolute linear thick lobes. *Berry* of a semipellucid reddish-yellow colour, ovate-oblong, two-seeded. *Seeds* oblong, contained in a tough integument, with the embryo in the axis of the albumen.

"I am not aware that these two plants have been described by any botanist since the time of Rumphius, or that any conjecture has been made regarding their place and family from his figure or description. From their common habit as parasites, I should have been much inclined to place them under one genus; but the different number of seeds in each, supported by the difference of a simple and bifid stigma, seems to oppose this, while the distinction is further confirmed by the different disposition and insertion of the leaves, which in *Hydnophytum* are arranged precisely as usual in the *Rubiaceæ*, but in *Myrmecodia* are crowded round the thick fleshy branches in such a manner, that their being really opposite is not immediately apparent, while their insertion on their broad peltate bases is further peculiar."

LASIANTHUS.

Rubiaceæ. Juss.

Calyx 4-partitus, laciniis linearibus. *Corolla* infundibuliformis, pilosa. *Stamina* 4. *Stigmata* 4, linearia, crassa. *Bacca*, tetrapyrena.

Suffrutices, floribus axillaribus, bracteis oppositis, baccis cyaneis.

Lasianthus cyanocarpus.

Villosus, bracteis magnis cordatis. Found at Tappanooly on the west coast of Sumatra.

Lasianthus attenuatus.

Villosus, foliis supra glabris, bracteis lanceolatis. Found in the interior of Bencoolen.

HELOSPORA.

Tetrandria Monogynia. Linn. *Rubiaceæ*. Juss.

Calyx 4-dentatus. *Corolla* tubulosa, limbo 4-partito. *Stamina* inclusa. *Stylus* 4-sulcus, apice 4-fidus. *Stigmata* quatuor.

Bacca calyce coronata, polysperma, seminibus duplici serie cruciatim dispositis, nidulantibus, linearibus, parum curvis.

Arborescens, glabra, pedunculis axillaribus unifloris, æstivatione valvata.

HELOSPORA FLAVESCENS.

Native of Sumatra.

"The disposition of the seeds in this genus is very peculiar, and forms a good distinctive character."

GLAPHYRIA.

Icosandria Monogynia. N. O. Myrtaceæ.

Calyx superus, quinque-fidus. *Corolla* pentapetala. *Bacca* quinque-locularis, polysperma; singuli loculi semina duplici ordine axi affixa.

Arbusculæ, foliis alternis, floribus axillaribus.

GLAPHYRIA NITIDA.

Foliis obovatis obtusis. Found on the summit of Gunong Bunko, or the Sugarloaf Mountain, in the interior of Bencoolen.

"This is a very handsome shrub, having much the habit and foliage of the common Myrtle, but the leaves are smaller and firmer. Its character and appearance are alpine, and it is only met with at high elevations; I found it on the summit of the Sugarloaf, and I am informed that it is almost the only shrub met with towards the top of the volcanic cone of Gunong Dempo in Passumah, where it is called *Kayo Umur panjang*, or the Tree of long Life, probably from its maintaining itself at elevations where the other denizens of the forest have ceased to exist. At Bencoolen an infusion of the leaves is drunk as a substitute for tea; and it is known to the natives by the name of the Tea Plant."

GLAPHYRIA SERICEA.

Foliis lanceolatis acuminatis. Found on Pulo Penang, an island on the western coast of Sumatra.

A plate accompanies this paper, showing the parts of fructification and the fruit, of *Lansium domesticum, Leuconotis anceps,* and *Helospora flavescens.*

VIII. *Description of the Cermatia longicornis and of three new Insects from Nepaul.* By Major-General Thomas Hardwicke, FRS. and LS. &c.

Cermatia longicornis.

Scolopendra longicornis. *Fab. Ent. Syst.* ii. 390. Scutigera longicornis. *Latr. Hist. Nat. des Crust. et Ins.* vol. vii. p. 89. Scutigera lineata? *Latr. Dict. d'Hist. Nat.* vol. xxx. p. 446.

"Body, when viewed beneath, having sixteen segments,

which are united above by eight unequal scuta. *Antennæ* of a pale colour, as long as the body, finely setaceous with three principal joints, each of which is numerously articulated. *External maxillary feet* or mandibles strong, subulate, incurvate, four-jointed. *Maxillary palpus* four-jointed, hairy, or rather spinulose, longer than the mandibles. *Eyes* large, hemispherical. *Feet* very long, fifteen on each side, with the last pair twice as long as the others. The principal articulations of the legs, viz. the two femoral joints and the tibiæ, are armed with stiff setæ. The *tibiæ* are flattened, angular, and of a pale colour, marked with transverse bands of a blueish-black. The *tarsi* are filiform, numerously articulated, and ending with a single subulate claw; and, with the exception of the hinder pair, which are transversely banded like the tibiæ, are of a pale-yellow colour.

"The longest specimen hitherto examined was one inch and a quarter in length from the base of the antennæ to the tail. Antennæ one inch and a half; and posterior legs $2\frac{1}{10}$ inches.

"This insect is found in damp houses under floor mats in all parts of Bengal, Bahar, and Orissa, but mostly during the rainy season, as Illiger has observed of his *C. lineata*. When living, the colours of the back and legs are bright, and varied between yellow, black, and brown; and although the above description by no means corresponds with the *Cermatia livida* described by Dr. Leach in the third volume of the *Zool. Miscellany*, it appears to answer to that of the *Scolopendra longicornis* of Fabricius."

Ord. *Neuroptera*. Fam. *Panorpidæ*. Genus *Panorpa*. Linn.

Panorpa furcata.

P. rufa, antennis nigris, alis hyalinis: superioribus puncto marginali fasciâ furcatâ apiceque nigris.

Ord. *Hemiptera*. Fam. *Gerridæ*. Genus *Gerris*. Latr. *Cimex*. Linn.

Gerris laticaudata.

G. rufa, antennis tarsisque nigris, caudâ utrinque bidentatâ supra unguiculatâ infra penicillatâ.

Ord. *Diptera*. Fam. *Tabanidæ*. Genus *Pangonia*. Latr.

Pangonia longirostris.

P. villosa flava, thorace ferrugineo, abdomine nigro-brunneo: segmentorum marginibus flavis, alis immaculatis.

"Length of the insect from the base of the rostrum to the apex of the abdomen ten lines; and of the rostrum two inches and a half."

The descriptions are illustrated by figures of each insect.

IX. *The Natural History of Phasma cornutum, and the Description of a new Species of Ascalaphus.* By the Rev. Lansdown Guilding, BA. FLS. &c.

Phasma cornutum.

P. cinereo-rufescens, capite cornuto; pedibus inermibus, angulatis, subæqualibus. *Mas.* Filiformis, pedibus fusco-fasciatis. Phasma filiforme. *Lich. in Act. Soc. Linn.* tom. vi. p. 9, tab. 1, f. 1, pessima. Mantis filiformis. *Gmel. Syst. Nat.* p. 2048, n. 15? *Fab. Ent. Syst.* tom. ii. p. 12? *Mant. Ins.* i. p. 227, n. 1? Phasma filiformis. *Fabr. Suppl.* p. 186? *Browne Hist. Jamaicæ,* p. 433, t. 42, f. 5. Hæc synonyma difficillima, quum nomen "filiforme" vix specificum, sed potius Phasmatum apterorum maribus subgenericum. *Fœmina.* Mare fere duplo major, fasciis femoralibus indistinctis. P. cornutum. *Lich. in Act. Soc. Linn.* tom. vi. p. 10. P. cornutum. *Stoll. Mant.* t. 13, f. 51.

Descriptio. Corpus elongatum, granulatum. *Oculi* parvi, prominuli. *Capitis* cornicula auriformia. *Oviductus* cymbiformis. *Pedes* æquales, femoribus ad basin dilatatis, articulo tarsorum longissimo triangulari. Coloris varietates multæ: sed ♂ et ♀ sæpiùs cinereo-nigri. Phasma cujus vita hic patet inter species confusas memorandum. Sexus copula vinctos iterum iterumque observavi, quamobrem nomen "filiforme" fœminæ maturæ nullo modo accommodatum omnino neglexi.

Ascalaphus Macleayanus.

A. alis vitreo-iridescentibus, immaculatis: oculis thoraceque cupreo-nigri: dorso maculato: ventre cinereo.

This communication is likewise illustrated with engravings of the insects described.

X. *On the Generic and Specific Characters of the Chrysanthemum Indicum of Linnæus, and of the Plants called Chinese Chrysanthemums.* By Joseph Sabine, Esq. FRS. FLS. &c.

"In a former communication* to the Linnean Society," Mr. Sabine observes, "I endeavoured to establish the correctness of my opinion, that the plants now cultivated in our gardens under the name of Chinese Chrysanthemums, had been improperly referred to the *Chrysanthemum Indicum* of Linnæus. Since the paper alluded to was written, I have had opportunities of examining and comparing living specimens of what I consider the real *Chrysanthemum Indicum* with those of the Chinese Chrysanthemum; which latter I now design to characterise as a distinct species under the name of *Chrysanthemum Sinense.*"

Chrysanthemum Indicum.

C. foliis flaccidis petiolatis pinnatifidis crebrè dentatis;

* Observations on the *Chrysanthemum Indicum* of Linnæus, vol. xiii. p. 561.

supremis integerrimis, radio calyce paulo longiore, caule fruticoso. Chrysanthemum Indicum. *Linn. Sp. Pl.* vol. ii. p. 889.—*ed.* 2, vol. ii. p. 1253. *Persoon Syn.* vol. ii. p. 461. *Willd. Sp. Pl.* vol. iii. p. 2147. *Sabine in Trans. Hortic. Soc.* vol. iv. p. 326, cum figuris. *Habitat* in Chinâ.

Chrysanthemum Sinense.

C. foliis coriaceis petiolatis sinuato-pinnatifidis dentatis glaucescentibus, radio longissimo, caule fruticoso. Chinese Chrysanthemum. *Sabine in Trans. Hortic. Soc.* vol. iv. p. 326.—vol. v. p. 149.—*in Trans. Linn. Soc.* vol. xiii. p. 561. *Habitat* incultum in Japonia (*Kæmpfer, Loureiro*); cultum (multis varietatibus) in hortis Sinarum atque Japoniæ.

"I am aware that an objection may be urged to the specific name I have applied to these plants, on the ground of their being natives of Japan, and only known in China in the gardens. But in reply to it, I should observe, that they were originally obtained from China, and we know it is in that country that they have been brought to their present state of beauty and perfection: that for these reasons they are now known all over Europe as the *Chinese Chrysanthemums;* and that, as they have hitherto been confounded with the *C. Indicum,* it is very desirable they should be distinguished by an appellation well opposed to that of the other species."

XI. *Descriptions of Seven new British Land and Fresh-water Shells, with Observations upon many other Species, including a List of such as have been found in the County of Suffolk.* By the Rev. Revett Sheppard, FLS.

The following are Mr. Sheppard's introductory remarks in this paper:—

"In the Descriptive Catalogue of British *Testacea,* published by Dr. Maton and Mr. Rackett, in the eighth volume of the *Linnean Transactions,* the *habitats* of the Land and Fresh-water Shells having for the most part been confined to the midland and western counties, I have been induced to lay before the Society a description of seven new species, and a list, with copious observations, of the Land and Fresh-water Shells hitherto discovered in the county of Suffolk, and occasional notices of places in which they have been found in Essex;* by which it will be seen, that the eastern parts also of this island are equally fertile in those elegant and interesting productions of Nature. The utility of such an undertaking seems to be generally allowed; and should this humble attempt meet with approbation from the lovers of conchology, I shall be amply gratified.

"Although I have followed Linnæus's arrangement in preference to any other, from the opinion that the Land and Fresh-water

* "My knowledge of Essex is confined to the hundred of Tendring, a peninsula formed by the German Ocean and the rivers Stour and Colne."

Shells are all reducible to his genera; I must nevertheless, in justice to M. Draparnaud, remark, that I esteem his work to be a most admirable one; and that his genera (at least those adopted by him), considering them as subdivisions of the Linnæan genera, are, with few exceptions, *secundum naturam.*"

Arrangement of the Suffolk Land and Fresh-water Shells.

TELLINA.	1. cornea	CYCLAS *of Draparnaud* and *Lamarck.*
	2. stagnicola	
	**	
	3. amnica	
	4. Henslowana	
MYTILUS.	5. cygneus	ANODONTA, *Drap.* and *Lamarck.*
	6. anatinus	
	7. Macula	
BULLA.	8. fontinalis	PHYSA, *Drap.* and *Lamarck.*
	9. hypnorum	
BUCCINUM.	10. terrestre	BULIMUS, *Drap.* ACHATINA, *Lam.*
TURBO.	11. viviparus	PALUDINA, *Lam.* CYCLOSTOMA, *Drap.*
	12. achatinus	
	13. tentaculatus	
	14. elegans	CYCLOSTOMA, *Dr.* and *Lam.*
	15. fontinalis	
	16. Leachii	
	**	
	17. laminatus	CLAUSILIA, *Drap.* and *Lamarck.*
	18. nigricans	
	***	
	19. Carychium	AURICULA, *Drap.* and *Lamarck.*
	****	
	20. tridens	PUPA, *Lamarck.*
	*****	
	21. perversus	PUPA, *Drap.* and *Lamarck.*
	22. muscorum	
	23. marginatus	
	24. Offtonensis	
	25. sexdentatus	

******

	26. nautileus	PLANORBIS, *Drap.* and *Lamarck.*

****

	27. cristatus	VALVATA; *Drap.* and *Lamarck.*
HELIX.	28. planorbis 29. planata 30. complanata 31. vortex 32. cornea 33. spirorbis 34. contorta 35. Draparnaudi 36. alba 37. fontana	PLANORBIS, *Drap.* and *Lamarck.*

**

	38. Somershamiensis	HELIX, *Drap.*
	39. lapicida	CAROCOLLA, *Lamarck.*
	40. paludosa 41. ericetorum 42. virgata 43. caperata 44. rufescens 45. Cantiana 46. nitens 47. nitidula 48. hispida 49. radiata 50. Kirbii 51. trochiformis 52. crystallina 53. spinulosa 54. arbustorum 55. nemoralis 56. hortensis 57. aspersa	HELIX, *Drap.* and *Lamarck.*

***

	58. Lackhamensis 59. obscura 60. lubrica	BULIMUS, *Drap.* and *Lam.*

****

	61. putris	SUCCINEA, *Drap.* and *Lam.*

	*****	
	62. stagnalis	
	63. palustris	
	64. fossaria	Limneus, *Drap.*
	65. limosa	Lymnæa, *Lamarck.*
	66. auricularia	
	67. lutea	
	******	
	68. pellucida	Vitrina, *Drap.* and *Lam.*
Nerita.	69. fluviatilis	Nerita, *Drap.* / Neritina, *Lamarck.*
Patella.	70. oblonga	Ancylus, *Drap.* and *Lam.*

Tellina stagnicola.—T. testa rhombea glabra, umbone exserto. Cyclas calyculata, *var.* 2. *Lamarck, Anim. sans vert.* v. 559. *Habitat* in stagnis. Testa $4\frac{1}{4}$ lin. longa, $5\frac{1}{2}$ lin. lata, glabra, tenuis, pellucida, cornei coloris, epidermide nulla. Valvulæ versus marginem complanatæ.

Tellina Henslowana.—T. testa oblique subovata transversim vix sulcata, projecturâ a basi umbonis adornata. *Habitat* in rivis. Testa 2 lin. longa, $2\frac{1}{2}$ lin. lata, cornei coloris, glabra, striata, vix sulcata, anteriùs planiuscula.

Turbo Leachii.—T. testa imperforata subovata, anfractibus 5 rotundatis oblique decurrentibus, sutura conspicua, apertura suborbiculari, operculo membranaceo. *Habitat* in rivis. Testa 3 lin. longa, $1\frac{3}{4}$ lin. lata, cornea, diaphana, glabra. Anfractus 5, teretes. Spira elongata. Apex acutus.

Turbo Offtonensis.—T. testa fusca striata subpellucida, anfractibus septem secundis sensim minoribus, aperturâ rotundatâ edentulâ nec marginatâ. *Habitat* super gramina et arbusta in sylvis, super truncos arborum, atque inter folia putrescentia. Testa plusquam $1\frac{1}{4}$ lin. Angustior quam *T. muscorum* et *T. marginatus*, et spiris sensim minoribus. Apertura edentula, margine nec reflexo, nec diverso colore.

Helix Draparnaudi.—H. testa supra subconcava subtus concava subcarinata, anfractibus quatuor transversim striatis: ultimo, majore. *Habitat* in aquis dulcibus. Testa diametro 3 lin. supra grisea, subtus albida, nitescens, anfractibus quatuor, ultimo, in medio juxta aperturam, subcarinato. Apertura dilatata.

Helix Somershamiensis.—H. testa grisea umbilicata, anfractibus 2 vix 3 reticulatis. *Habitat* in sylvis, rarissima. Equal in magnitude to a middle-sized *H. alba*, which it resembles in shape; is of a greyish colour, and curiously reticulated, particularly above.

Helix Kirbii.—H. testa nunc subconica nunc subdepressa subpellucida striata, anfractibus quatuor, umbilico patulo. *Habitat* sub saxis et lignis. Testa diametro $\frac{1}{2}$ lin. rufo-cornea; anfractibus subtiliter striatus. Apertura subrotundo-lunata. Labium tenue. Umbilicus profundus.

Article XIII.

Proceedings of Philosophical Societies.

METEOROLOGICAL SOCIETY OF LONDON.

On the 15th of October, a Meeting was held at the London Coffee House, Ludgate-hill, to take into consideration the propriety of forming a Meteorological Society. Among the gentlemen present were Drs. T. Forster, Clutterbuck, Shearman, Mr. Luke Howard, &c.: at eight o'clock the Chair was taken by Dr. Birkbeck, when the following Resolutions were agreed to:—

1. *Resolved,* That the formation of a Society to promote the advancement of Meteorology, have the cordial approbation of this Meeting.

2. *Resolved,* That a Society be formed to be called "The Meteorological Society of London."

3. *Resolved,* That the business of this Society shall be conducted by a President, Vice-Presidents, Treasurer, Secretary, and Council; and that the number of Vice-Presidents and Members of the Council be determined at a subsequent Meeting.

4. *Resolved,* That Mr. Thomas Wilford be requested to officiate as Secretary to this Society (*pro tempore*), and that he be authorized to send a printed summons to attend the next Meeting to each person who shall become a Subscriber.

5. *Resolved,* That an Annual Subscription of Two Guineas be paid in advance by every Member of this Society.

6. *Resolved,* That those gentlemen present who are inclined to become Members of this Society, do now send their names to the Secretary to be enrolled.

7. *Resolved,* That a Committee of three Members be appointed, in conjunction with the Secretary, to draw up an account of the Society's proceedings this evening.

8. *Resolved,* That scientific men throughout the United Kingdom are solicited to co-operate with this Society, and to transmit communications to it; and that this Society will always be ready to receive meteorological observations from the cultivators of science throughout the various quarters of the globe.

9. *Resolved,* That no other qualification be required to constitute eligibility to this Society, than a desire to promote the science of Meteorology.

10. *Resolved,* That after the next Meeting the election be by ballot upon the proposition of three, and that a majority of Members decide.

11. *Resolved,* That this Meeting do adjourn to the 12th of November next, to meet at the same place and hour.

MEDICO-BOTANICAL SOCIETY OF LONDON.

This Society held its first meeting this Session on Friday, Oct. 10.

An address was delivered to the members on the objects and utility of the Institution; after which the death of its late Honorary Member, Dr. Baillie, was notified to the Society, accompanied by an appropriate eulogium on his character.

The meeting then adjourned to Oct. 31, 1823.

Article XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Return of the Expedition for the Discovery of a North-west Passage.*

Our readers have doubtless been apprised, through the public papers, of the safe return of the Expedition under Capt. Parry. The primary object of the voyage, it appears, has not been attained; the only channel through which a passage to the westward was to be expected, after it had been ascertained that the openings in Repulse Bay and its neighbourhood were mere inlets to the American continent, being blocked up by ice throughout the year. No particulars have as yet transpired respecting the scientific results of the Expedition, which we deem sufficiently authentic for their transfer to the pages of the *Annals.*

II. *Solar Light and Heat.*

Mr. Powell has been for some time engaged in experiments on solar light and heat. He has examined the heating power of the prismatic rays, but chiefly with respect to the effects said to be produced beyond the red end of the spectrum. He has found that such effects are really produced; but has accounted for their being observed in some cases, and not in others, from certain differences in the coatings of the thermometers employed. He has concluded from a number of experiments with different coatings, that this heating effect is similar in its relations to surfaces, to common radiant heat; and differs essentially in this respect from the heating power *within* the spectrum. He has made other experiments from which the nature and origin of this effect may with great probability be inferred. The details will soon be made public.

III. *On Cleavelandite.*

From the examination Mr. Levy has recently made of the felspars contained in Mr. Turner's collection, it appears that half the specimens which have hitherto been ranked under this name, belong to the species which had been called albite, and has recently received the name of cleavelandite from Mr. Brooke. It is rather curious that the crystallographical difference between this last substance and felspar, should have been detected upon specimens laminated, but not regularly

crystallised, and that the many crystals which it presents should not have been noticed. The varieties of forms of cleavelandite are, however, at least as numerous as those of felspar; the crystals are very distinct, of various sizes, but rather large than small; they are very frequently marked parallel to one of the primitive planes, viz. that which is the least easy to obtain by cleavage. Several of the forms greatly resemble some of the varieties of felspar, being composed of the same number of planes disposed in the same manner, and it is only by using the goniometer that the difference can be perceived. Notwithstanding this great analogy, Mr. Levy believes that the forms of the two substances are incompatible. He considers the primitive of felspar to be an oblique rhombic prism, and not a doubly oblique prism, as it had been supposed by Hauy, and he takes for the primitive of cleavelandite a doubly oblique prism. The crystals of cleavelandite are generally white, sometimes yellowish and reddish; they are transparent, sometimes translucent and opaque, and have a certain brilliancy which does not belong to felspar. Both substances are often found upon the same specimen, and sometimes both in large and well defined crystals. The localities of cleavelandite are very numerous, and this substance seems likely to become one of the most important both in mineralogy and geology. All the rocks of which felspar is considered as a component part, must be re-examined to separate those which really contain felspar, from those which contain cleavelandite. The localities derived from Mr. Turner's collection are the following: Dauphiny, St. Gothard, Tyrol, Piedmont, Baveno, Elba, Vesuvius, Saxony, Sweden, Norway, Siberia, Greenland, United States, and Rio di Janiero.*

The finest crystals come from the Tyrol and from St. Gothard. The largest from Siberia, where they are met upon the same specimen with large crystals of reddish felspar, and smoky quartz. The most transparent come from Dauphiny, where they are met in small transparent, brilliant, macled crystals, with chlorite, quartz, and occasionally felspar *ditetraèdre.* Specimens of this locality are very commonly met with in collections, and the crystals they contain were described by Hauy as felspar *quadridécimal.* At Baveno, it sometimes forms the guangue of the fine flesh-coloured crystals of felspar. From Greenland there is a lamellar, *chatoyante* variety which greatly resembles the moon stone. However, the moon stone from Ceylon does not belong to cleavelandite; it gives easily two cleavages at right angles like felspar. The other cleavages Mr. Levy could not obtain, and what is very remarkable, the direction of the laminæ which give the beautiful chatoyant reflection of light, corresponds to no cleavage of felspar, nor to any of the secondary planes observed in that substance.

IV. *Change of Musket Balls in Shrapnell Shells.*

Mr. Faraday states, that " Mr. Marsh, of Woolwich, gave me some musket balls, which had been taken out of Shrapnell shells. The shells had laid in the bottom of ships, and probably had sea water among them. When the bullets are put in, the aperture is merely closed by a common cork. These bullets were variously acted upon: some were affected only superficially, others more deeply, and some were entirely

* Mr. Levy proposes soon to publish more minutely the result of his observations; and the exact localities of each specimen will be given.

changed. The substance produced is hard and brittle, it splits on the ball, and presents an appearance like some hard varieties of earthy hæmatite; its colour is brown, becoming, when heated, red; it fuses, on platinum foil, into a yellow flaky substance like litharge. Powdered and boiled in water, no muriatic acid or lead was found in solution. It dissolved in nitric acid without leaving any residuum, and the solution gave very faint indications only of muriatic acid. It is a protoxide of lead, perhaps formed, in some way, by the galvanic action of the iron shell and the leaden ball, assisted, probably, by the sea water. It would be very interesting to know the state of the shells in which a change like this has taken place to any extent; it might have been expected, that as long as any iron remained, the lead would have been preserved in the metallic state."—(Institution Journal, for Oct. 1823.)

V. *Action of Gunpowder on Lead.*

Mr. Faraday says, that "Mr. Marsh gave me also some balls from cartridges about fifteen years old, and which had probably been in a damp magazine. They were covered with white warty excrescences rising much above the surface of the bullet, and which, when removed, were found to have stood in small pits formed beneath them. These excrescences consist of carbonate of lead, and readily dissolve with effervescence in weak nitric acid, leaving the bullet in the corroded state which their formation has produced. It is evident there must have been a mutual action among the elements of the gunpowder itself, at the same time that it acted on the lead; and it would have been interesting, had the opportunity occurred, to have examined what changes the powder had suffered."—(Ibid.)

VI. *Purple Tint of Plate Glass affected by Light.*

"It is well known," says Mr. Faraday, "that certain pieces of plate glass acquire, by degrees, a purple tinge, and ultimately become of a comparatively deep colour. The change is known to be gradual, but yet so rapid as easily to be observed in the course of two or three years. Much of the plate glass which was put a few years back into some of the houses in Bridge-street, Blackfriars, though at first colourless, has now acquired a violet or purple colour. Wishing to ascertain whether the sun's rays had any influence in producing this change, the following experiment was made:—Three pieces of glass were selected, which were judged capable of exhibiting this change; one of them was of a slight violet tint, the other two purple or pinkish, but the tint scarcely perceptible, except by looking at the edges. They were each broken into two pieces, three of the pieces were then wrapped up in paper, and set aside in a dark place, and the corresponding pieces were exposed to air and sunshine. This was done in January last, and the middle of this month (September), they were examined. The pieces that were put away from light seemed to have undergone no change; those that were exposed to the sunbeams had increased in colour considerably; the two paler ones the most, and that to such a degree, that it would hardly have been supposed they had once formed part of the same pieces of glass as those which had been set aside. Thus it appears that the sun's rays can exert chemical powers even on such a compact body and permanent compound as glass."—(Ibid.)

VII. *Test of Platinum.*

Prof. Silliman recommends the hydriodic acid, as the best test for platinum in solution. When dropped into a weak solution, it almost immediately produces a deep wine red, or reddish-brown colour, which by standing grows very intense. It resembles the effect of muriate of tin, but is more sensible. On remaining a day or two, films of platinum were deposited. The hydriodic acid had been prepared, by putting phosphorus to about an equal bulk of iodine, placed under water in a glass tube, so that it remained mixed with acids of phosphorus, and perhaps phosphorus itself. No other metallic solution gave similar results.—(Silliman's Journal, vi. 376.)

VIII. *Westbury Altitude and Azimuth Instrument.*

To most of our astronomical readers it is probably known, that on the return of the Westbury circle to London during the last winter, it was found in a state "unfit for any nice astronomical purpose:" it has, however, under the superintendance of Mr. Troughton, undergone a complete repair; to secure the telescope from flexure, its original object glass of 2¾ inches aperture, and 43 inches focus, has been replaced by one of the like diameter, but whose focal length is 38 inches only; it separates many of the close double stars, shows distinctly the double ring and belts of Saturn, and was made by Mr. Tully. The artist who has had the immediate management of the repairs is Mr. Simms, of Bowman's-buildings, Aldersgate-street, and we are glad to know that an instrument which has rendered such essential service to astronomical science is again fit for immediate use. We quote the inscription it now bears with pleasure. "With this instrument, the work of Edward Troughton, Mr. Pond substantiated the errors of the Greenwich mural quadrant; the observations were made at Westbury, and are recorded in the Philosophical Transactions. The instrument, having suffered from long exposure to the weather, was repaired and redivided for Mr. South, by William Simms, under the direction, and to the satisfaction, of its illustrious maker."—Aug. 10, 1823.

IX. *Correctness of Greenwich Observations.*

For some time past we have seen with regret the various attempts which have been made by certain closet astronomers to withdraw the confidence of the public from observations made at the Royal Observatory, and we have waited with much anxiety for the period, when their accusations, and still more dangerous insinuations, should be repelled. That time, we rejoice to say, is arrived; a communication has, we understand, been received from Mr. Bessel, acknowledging that his Catalogue of Principal Stars requires a Correction for Instrumental Flexure; thereby admitting the superiority of the Greenwich one. For this distinguished foreign astronomer we entertain the highest respect; but, when his observations differed so seriously with those made at our own great national establishment, we hesitated not which to confide in; and we are glad our confidence has not been misplaced. To such as have most distinguished themselves by their *patriotic* endeavours to depreciate the labours of their countrymen, we would offer the following advice:—Use your pens less freely; your instru-

ments more frequently; give us results from your observatories, rather than surmises from your closets; and, should fresh discordances arise between the observations made at home and abroad, you may, *perhaps*, in process of time, be called upon to settle the dispute. X.

Article XV.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

A Treatise on Organic Chemistry, containing the Analyses of Animal and Vegetable Substances; founded on the Work of Prof. Gmelin on the same subject. By Mr. Dunglison, Member of several Learned Societies, Foreign and Domestic, and one of the Editors of the "Medical Repository."

Dr. Henderson's History of Ancient and Modern Wines, in an elegant Quarto Volume, embellished with Vignettes, and other decorative Wood-cuts from the Antique.

A Treatise on Navigation and Nautical Astronomy; adapted to Practice, and to the Purposes of Elementary Instruction. By Mr. Riddle, Master of the Mathematical School, Royal Naval Asylum.

Observations on the Functions of the Digestive Organs, especially those of the Stomach and Liver. By Dr. Prout.

Naval Battles from 1744 to the Peace in 1814. Critically revised and illustrated. By Admiral Ekins.

A Guide to Practical Farriery; containing Hints on the Diseases of Horses and Neat Cattle, with many valuable and original Recipes from the Practice of an eminent Veterinary Surgeon. By Mr. Pursglove, sen.

Article XVI.

NEW PATENTS.

J. Hughes, Barking, Essex, slopseller, for certain means of securing the bodies of the dead in coffins.—Sept. 11.

H. C. Jennings, Devonshire-street, Marylebone, for an instrument to be affixed to the saddle-tree, by the application and use of which, inconvenience and distress to the horse may be avoided.—Sept. 11.

J. Sprigg, sen. Birmingham, fender-maker, for a certain improvement in the manufacture of grates, fenders, and fire-iron rests. Sept. 11.

T. Wickham, Nottingham, lace-manufacturer, for his improved and prepared rice, rendered applicable for use in all cases in which starch is applied.—Sept. 11.

W. Hase, Saxthorpe, Norfolk, iron-founder, for his method of constructing mills or machines chiefly applicable to prison discipline.—Sept. 11.

Article XVII.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
9th Mon.								
Sept. 1	S W	30·32	30·12	74	50	—		
2	S W	30·16	30·12	75	46	—		
3	W	30·26	30·16	68	54	—		
4	W	30·26	30·24	77	50	—		
5	N W	30·24	30·22	68	48	—		
6	N W	30·28	30·24	72	40	·84		
7	N E	30·38	30·28	68	35	—		
8	N E	30·38	30·35	68	33	—		
9	N	30·35	30·33	69	33	—		
10	N	30·38	30·33	71	41	—		
11	E	30·33	30·26	71	40	—		
12	E	30·26	30·02	76	51	—		
13	S	30·02	29·92	77	51	·75		
14	S W	29·92	29·50	76	63	—	02	
15	W	29·94	29·50	68	48	—	—	
16	S W	30·02	29·94	71	52	—	—	
17	S W	30·42	30·02	64	36	—	07	
18	N	30·43	30·40	68	34	—		
19	N W	30·40	30·25	71	50	—		
20	N W	30·25	30·02	61	40	—		
21	S W	30·02	29·46	60	48	·86	19	
22	N W	30·07	29·46	68	41	—	11	
23	N W	30·07	29·97	58	48	—	21	
24	N W	30·10	29·97	73	52	—		
25	W	30·10	29·98	61	54	—		
26	W	29·98	29·91	66	44	—		
27	N W	30·01	29·93	66	29	—		
28	N W	30·11	30·01	62	30	—		
29	N	30·11	29·58	64	31	—		
30	N	29·58	28·88	55	44	·90	1·05	
		30·38	28·88	77	29	3·35	1·65	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Ninth Month.—1—14. Fine. 15. A violent storm of hail and rain, accompanied by very vivid lightning, and a few claps of thunder, between three and four, a. m. 16. Fine. 17. Overcast. 18. Fine. 19. Foggy morning. 20. Fine. 21. Rainy. 22. Showery. 23. Fine: night rainy. 24, 25. Cloudy. 26—29. Fine. 30. Very rainy day with strong wind: a vivid flash of lightning, with a loud clap of thunder, between five and six, p. m.: a second flash, with thunder, about an hour afterwards.

RESULTS.

Winds: N, 5; NE, 2; E, 2; S, 1; SW, 6; W, 5; NW, 9.

Barometer: Mean height

For the month.................................... 30·075 inches.

For the lunar period, ending the 27th................ 30·109

For 13 days, ending the 5th (moon north)........... 30·170

For 14 days, ending the 19th (moon south)........... 30·173

Thermometer: Mean height

For the month.. 56·033°

For the lunar period.................................. 57·172

For days, the sun in Virgo............................ 59·290

Evaporation... 3·35 in.

Rain.. 1·65

Laboratory, Stratford, Tenth Month, 21, 1823. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1823.

Article I.

Remarks on different Gas Works, and the Substances from which Gas is usually prepared. By Timothy Dewey, Esq. of New York.

(To the Editor of the *Annals of Philosophy.*)

[Mr. Dewey, during his mission to this country, for the purpose of acquiring information on the subject of gas lighting, having visited numerous establishments, I requested him to favour me with answers to a few questions on particular points, and to some of them he has replied in the annexed communication.—*Edit.*]

DEAR SIR, *Nov.* 9, 1823.

The note you addressed to me on the 7th inst. was duly received, and the following hasty sketch must be my reply. I have assisted in making experiments on coal and oil gas at Whitechapel-road, and think it a duty to assist in correcting a prevailing error respecting the illuminating power of oil gas, on which must depend its relative value compared with coal gas. You have given me an opportunity to do so, and I thank you for it. I was deputed by a highly respectable Company in the city of New York to examine the gas works in this country, preparatory to constructing one for lighting that city. The time I have been able to bestow on the subject has been much too short to allow me to acquire a minute practical acquaintance with the subject in all its details. I have visited many gas works in Great Britain, and several in France, and have the satisfaction to state, that I have received the most flattering attention from the proprietors and managers of all the works I

have seen, and in many instances they vied with each other in their endeavours to be most useful to me. With such facilities and assistance, I ought tolerably well to understand the *outlines* of the systems of gas-lighting. I must rely on practice to perfect me in the minor and more complicated parts. The first work I examined was the coal gas work at Liverpool, which is under the management of Mr. John King, a gentleman who has, by his activity, intelligence, and practical experience, overcome many formidable difficulties. Superior light is afforded at a cheap rate, and the stock is held in high estimation; I have seen no work better conducted. The coal gas work at Dublin has had to contend with the prejudices of the day. At most coal gas works the sale of the coke is one principal source of profit. In Dublin, sales of coke cannot be made to any extent, though it is offered at a very low price. This work is managed by Mr. John Brunton, who is every way qualified for the station, and he explained every thing to me which he supposed useful. An oil gas work was erecting when I was there, which has since gone into operation with flattering prospects of success. The high price of coals, and want of sale for the coke by the coal gas work, will enable the oil gas to compete with the former. A work was nearly completed at Belfast, which, in design and workmanship, surpasses any I have seen. Coals are obtained from Newcastle at lower prices than in Dublin. They commenced lighting the town the 1st of September, and cannot fail to succeed.

From Belfast I went to Glasgow. Here is a very extensive work under the excellent management of Mr. James B. Neilson, engineer, assisted by many scientific gentlemen of that city. This company supplies more than 40,000 single jet lights, and at a lower price than any work in the kingdom that I have visited. They have better coal, and furnish better coal gas than any I have seen. Their stock is as high as any in the country. I am greatly indebted to Messrs. J. B. Neilson, John Hart, Robert Hastie, and John Thompson, for giving me every assistance in forwarding the objects of my mission to this country. The purification of coal gas is quite as well understood here as at any work I have seen, and as well practised. It is on the wet, or cream of lime system, and is very effectual, considering the quantity of sulphur the coal contains. Mr. Neilson has discovered a method of discharging the hydrosulphuret of ammonia, which is so destructive to iron and copper pipes; but owing to the expense of the process, and the low price at which they furnish the gas to customers, it has not been carried fully into effect. Extensive additions are making to this work, and the manufacturers of that city are under many obligations to the Company for the increased facilities afforded by this pure and brilliant light to their various processes. I went from Glasgow to Edinburgh, where there is an extensive gas work, and well

conducted. They furnish gas for about 8000 Argand burners, and obtain a higher price than at Glasgow. I visited a small oil gas work at Leith which had been but a short time in operation. I could not form a just estimate of its probable success; I think it more likely to succeed than coal gas in so small a place. The consumption must be limited for a long time. I think that neither coal nor oil gas can be profitable in small towns, unless they are exclusively manufacturing ones. From thence I went to Hull, where one of the first large oil gas works was constructed. This is a small town without manufactures, and must principally rely on the merchants and shopkeepers for support. The consumption of gas is necessarily limited, and as yet the proprietors have not derived much profit from their investment. Oil is now much lower than for years past, and they are going on prosperously. A coal gas work could not succeed here. I examined the coal gas work at Leeds, which is under the particular care of Mr. J. B. Charlesworth, an eminent merchant of that town, who took great pains to instruct me. Here I found some important improvements in the method of heating the retorts, the condensation of the gas, and mode of purification. Coal is obtained here at a lower price than any where else I have been. The gas is passed through dry lime, and most effectually purified. I have yet to form an opinion of the best mode of purification. The dry lime, I think, is to be preferred where there are no ready means of getting rid of the waste lime and water, which are very offensive, and where lime is cheap. Where lime is dear, and the furnaces are constructed to evaporate the waste water, perhaps the cream of lime is preferable. The condenser is a straight main pipe, 12 inches diameter, and 72 feet long, laid on an inclined plane in a trunk, or canal, filled with water, and connected with the tar cistern, the gas is completely condensed in passing through this pipe. A small stream of water supplies this trunk, and keeps the pipe cool. The gas costs less to make it per 1000 feet at this work than at any I have examined. Dr. Hawksworth principally manages the gas work at Sheffield, and it is well managed. The improved mode of condensing is adopted here. The work is profitable, and should be in all manufacturing towns where coal can be had reasonably. Coal gas must be held in high estimation at Liverpool, Glasgow, Edinburgh, Leeds, Sheffield, Leicester, and in places where coal is equally cheap. I have examined many of the principal gas works in London. Here, where coal is dear, and the expences of making gas high, I think that had oil gas been first introduced, with the present improved method of manufacture, it would have answered the general purposes better than coal gas, and might be furnished at the same price for an equal quantity of light. But as it is, London is splendidly lighted, and I hear no complaints from customers; and those who do not patronise these works should not complain.

In the course of my investigations, I found opinions much at variance on the subject of the illuminating powers of the two gases, and I could not form an opinion at all satisfactory. To put the matter at rest, I proposed to Messrs. Taylor and Martineau to find a situation where the main pipes from coal gas and oil gas works run parallel to each other, and to bring the gases, as they were supplied to customers, into the same room, and burn them together. They found such a situation in the Whitechapel-road, and had them brought together, and the burners supplied through two accurately adjusted meters, made by Mr. Crossley, and who with Dr. Arnot, Mr. Preuss, and yourself, were invited to assist us; and Mr. Crossley kindly undertook to manage the meters that no mistake might occur, The coal gas was supplied from the mains of the Imperial Gas Works, and the Oil Gas from that at Bow. We then proceeded to make the experiments on two separate evenings.

The specific gravity of the two gases was found to be, of coal gas 0·4069,* of oil gas 0·9395; and the average consumption per hour (being the mean of seven experiments, and which varied but slightly from each other), coal gas 4·850 feet, oil gas 1·368 feet. The flames were adjusted so as to give a light of equal intensity. I have seen more than 100 feet of gas obtained from a gallon of good clean whale oil, and am confirmed in the opinion that about that average can be obtained, from an inspection of the books of four oil gas works. Oil gas has been objected to on account of its price. This must be owing to the opinion that one foot of oil gas does not contain as much illuminating matter as about three and a half feet of ordinary coal gas. But it does. I would not be satisfied till I had tried it, nor with one night's experiments. And was it not so, and suppose it to cost much more, which I do not admit where oil is cheap, and coals dear, I know no good reason that some people should not wear *superfine*, though the great mass of the world are satisfied with *fine;* and they will use *superfine* oil gas, and pay for it, most certainly, if they can obtain it as cheaply as *fine* coal gas. The

* These results coincide very nearly with those obtained a few months since by M. Faraday and myself, on comparing the specific gravity and illuminating powers of the two gases at Messrs. Hawes' and Co. soap work. The oil gas was manufactured on their premises, and the coal gas was obtained from a neighbouring establishment. Its lightness sufficiently proves that it was uncontaminated by any accidental admixture. I shall give at one view the results obtained at both places.

At Messrs. Hawes'.

	Coal gas.	Oil gas.
Specific gravity	0.4291	0.9657
Illuminating power	1.	3.567

So that one cubic foot of oil gas is equal to rather more than 3½ feet of coal gas.

At Whitechapel.

	Coal gas.	Oil gas.
Specific gravity	0.4069	0.9395
Illuminating power	1.	3.541

It will be observed, that in the latter case, although both gases were specifically lighter than in the former, their comparative illuminating power is nearly similar.—*Edit.*

coal gas works at Glasgow, Liverpool, Sheffield, Leeds, Leicester, Belfast, as well as London, and many other large commercial and manufacturing towns, as well as small ones, in Great Britain, will use coal gas, and some of them oil gas too, and the people will be greatly benefited by them. I see no good reason that they should not both do extremely well, though some *scientific*, not *practical* men, may try to make us believe, that *one* or the other is good for nothing. I see they are both good, and there is plenty of room for both in this great manufacturing country. I shall recommend to my employers to begin with oil gas, because it is the best; and oil being cheap, and coals dear, it will be the cheapest, and will not cost nearly so much to begin with as coal gas, and we shall require fewer hands to carry it on. I can more easily manage it. When I can get coals as cheaply as they do at Sheffield, and some other places, I may make coal gas too. Besides, I need not lay down pipes more than one-third as large as for coal gas, and this is a great saving; so we shall all save by it. You ask me to tell you how much oil gas costs, and how much profit may be made by it. I cannot tell you any thing about that till I have erected works, and made some oil gas, and see how many people use it, and what they will pay for it. I have given you more opinions already than some may think correct. If they desire to possess more information, I would advise them to travel as I have done, and inquire of every one they can find to tell them; and I am certain they will eventually acquire it. I think the gas-lighting system in its infancy even in this country; and to learn all about it, you must see all engaged in it. I have not seen half of them, but must go home and do the best I can.

You desire me to state what quantity of gas can be obtained from different kinds of coal. This depends so much on the manner in which it is worked, that if I give an opinion some will say it is too *high*, and others too *low*. At Liverpool, I think, Mr. King says he finds it good economy to obtain only about 7000 cubic feet from a ton. He uses the Wigan Orral coal. At Glasgow, they obtain from their rich candle coal 12,000 feet. This coal is called *candle* coal, because the people formerly (and now too for what I know), used it instead of candles. They make the best gas I have seen, always excepting that procured from the decomposition of oil. Mr. Peckston has given a very good table of the different kinds of coal in the kingdom, and the quantity of gas to be obtained from each; and had he seen as much of *oil* gas as I have, I think he would have given a better account of that than he has. To give my opinion regarding retorts, I think those used by Mr. King, and invented by him, the best I have seen for a large work. They are large, wide, and flat, shaped something like a D, turned half over to the left. They are made of rolled iron, and rivetted. Those at Glasgow are much smaller, of cast-iron, and nearly of the same shape. Mr. George Lowe, manager of the

Brick-lane and Dorset-street stations, in London, uses similar ones, and finds them answer better than the round ones. The former are both theoretically and practically the best. The coal is more equally and more speedily carbonized. The fire comes more readily in contact with every part. The thinner the coal is in the retorts the better. Perhaps you will say I have seen many more gas works than I have mentioned: true, and there are many who understand them better than I do. When I get home, and find I know enough to construct a work which shall answer our purposes, and the people like the gas, and use it, and pay as fairly for it, then I may send you another article, but not so long as this.

At Paris, I examined their coal gas works, and one oil gas work. Here I met with the most frank and gratifying treatment from Messrs. Say, Thenard, and Darcier, who interested themselves to procure me admission to the gas works; the managers of which showed me every thing, and treated me equally well. Gas works in Paris will long have to contend against the best lamps and purest oil I have seen. Light is not used nearly so late in Paris, except in the coffee-houses, as in England, which is a great drawback on this kind of industry. The oil gas here is obtained from oleaginous seeds, principally from the colza and hempseed; the quantity great, but the illuminating power not quite half (as nearly as I could determine by a few experiments), that from fish oil. It is not so offensive as coal or oil gas. I think this work will be profitable. One coal gas work may be profitable too. I understand that the largest is to be removed beyond the walls of the city. They have a gasometer which may contain 256,000 feet of gas, and a removal will destroy the concern. I believe many people are taught that such a large gasometer is dangerous. I do not think there is much fear of a gasometer's exploding. Some trifling explosions may occur in confined places, but they will never injure any body. They are not half so dangerous as most mechanical employments, or a windmill, which no one is afraid of.

I have not volunteered these opinions, or stated facts to serve any separate interest. Both systems can be successfully prosecuted, and that to be preferred must depend on so many local and contingent circumstances, that every one must judge for himself. My business was to acquire correct information on this interesting subject to enable me to give my employers the means of judging which system would best answer our particular situation; and as they had given me power to act for them to a certain extent, I have decided as above stated. I have been much longer absent than they expected; and I fear am not so well informed as they could wish.

I am happy to say that the statements made to me by Messrs. Taylor and Martineau, respecting the consumption and illuminating power of oil gas, have been verified by the experiments they enabled me to make. Were gas works now to be erected

to produce the same quantity of gas that is used, I think that nearly one-half the amount expended might be saved; such has been the progress of improvement, and that principally in simplifying the works.

I am most respectfully, your obedient servant,

TIMOTHY DEWEY.

ARTICLE II.

General Conclusions of an Inquiry into the Era when Brass was used in Purposes to which Iron is now applied. By the Rev. John Hodgson, Secretary of the Society of Antiquaries of Newcastle upon Tyne.*

General Conclusions respecting Iron.

1. Meteoric stones, consisting principally of iron in a malleable state, probably led mankind to the discovery of iron from its ores. To this day large balls of iron stone found in certain parts of Sicily, are called thunderbolts, a name they have no doubt received from their similarity in substance and shape to the true aërolite.†

2. The Egyptians, in the time of Moses, were well acquainted with the use of iron; and all the agricultural and mechanical implements of the Hebrews, from that age downwards, were of that metal. In the time of David they had it in the greatest plenty, as appears from the account of the immense quantity of it, which he provided for the temple, which his son built.

3. The Greeks supposed that iron was first discovered by the burning of wood upon Mount Ida, 1438 years before Christ. In the time of Homer and Hesiod it was scarce and valuable: but the account of the iron money of Lycurgus, and the extracts I have given from Herodotus and other authors, prove, that for more than 400 years before the Christian era, it was plentiful. The account derived from the Poliorcetica Commentaria of Daimachus, and contained under Lacedæmon in Stephanus, gives even the uses to which several kinds of iron were applied in edge tools.‡

* Extracted from an elaborate memoir on the subject in the Archæologia Æliana, or Transactions of the Newcastle Antiquarian Society, Part I.

† Some remarks explanatory of this passage of Mr. Hodgson's paper, will be found under the head "Scientific Intelligence," &c. in the present number of the *Annals*.

‡ "Different sorts of steel are produced amongst the Chalybes, in Sinope, Lydia, and Laconia. That of Sinope and the Chalybians is used in smith's and carpenter's tools; that of Laconia in files, drills for iron, stamps, and mason's tools; and the Lydian sort is manufactured into files, sabres, razors, and knives." (See Bochart's Phaleg. p 208.) Daimachus, of Platæa, lived before the time of Strabo. Plutarch has copied a very interesting account of a meteor that threw down stones, from a treatise, which this author left concerning religion. He also wrote something respecting India. See Solon and Publicola compared; the Life of Lysander, &c.

4. When Cæsar landed in Britain, all the nations of Europe enjoyed the advantages which arise from the use of steel; and the Britons had iron works of their own. It is probable too that the Egyptians or Phœnicians had made mercantile voyages to their country, more than sixteen centuries before that time That it was known to the Phœnicians in the time of Homer, his accounts of amber and tin are unquestionable evidence. And there can be no doubt, but that the Greeks and Romans frequented it commonly ever after the destruction of Carthage, if not sooner: Pliny indeed says, this country was in his time, "Clara Græcis nostrisque monumentis," and he wrote before the Romans were extensively settled in the country.* And besides their knowledge of iron, and their long intercourse with foreign and civilized nations, their old established tin trade is a proof that they had been accustomed to work in mines for numerous ages; and there is no account that implements of bronze are more abundantly found in the old mines and rubbish heaps of the tin districts, than in those parts of the country which are destitute of all sorts of mines.

5. If *κολλησις σιδηρευ* signify welding of iron, then we have a proof that malleable iron was in use at the time of Alyattes, king of Lydia.† Perhaps the different sorts of iron which Pliny calls *Stricturæ,* received their name from their being malleable, "a stringendo acie," from *binding the edge,* i. e. from having the property of welding, "quod non in aliis metallis." The sentence, "mollior complexus (i. e. ferri) in nostro orbe," probably alludes to the same property. But though two pieces of common iron, or a piece of iron and steel, by using siliceous sand, unite at a white heat more readily than two pieces of steel; yet very highly cemented steel may be readily and very perfectly welded by using finely powdered potter's clay instead of sand: and the ancients were acquainted with this process, as appears from Pliny, for in describing the solders used for different sorts of metals he says, "argilla ferro."

Conclusions respecting Bronze, Brass, &c.

1. Before the flood, Tubal-Cain (i. e. the possessor of the earth), was "an instructor of every artificer in brass and iron."

* Plautus, in A. D. 43, was the first of the Romans after Cæsar, who came into Britain as an invader, and Pliny died 35 years after that time.

† Alyattes, a king of Lydia, who died 562 years before Christ, made an offering at Delphi of "a silver cup, with a stand for it, made of iron welded together. It was as worthy of observation as any of the things at Delphi. It was the work of Glaucus the Chian who first of all found out the method of welding iron." (*σιδήρευ κόλλησιν*) "The joinings of this stand were not made with clasps or rivets, but welding was the only fastening. In form it nearly resembles a tower rising from a broader base into a narrow top. Its sides are not wholly continuous, but consist of transverse zones of iron, like the steps in a ladder. Straight and ductile plates of iron diverge from the top of each bar to the extremity." This stand was the only offering, made by the Lydian kings, which remained at Delphi in the time of Pausanias. (Herod, Clio. 25. Paus. Phoc. c. xvi. sec. 1)

Does this passage, besides affording us a valuable notice in the history of the useful arts, lead us to some knowledge in antediluvian geography. After the flood, Tubal and Mesech, sons of Japhet, settled on the borders of the Euxine Sea: In Ezekiel's time, their descendants traded to Tyre in "vessels of brass;" and by the Greeks were called Tibareni and Moschi.

2. Because Moses mentions metal mirrors and tin, I infer, that the Egyptians, before his time, were acquainted with the use of tin in hardening copper for edge-tools; consequently, that their most ancient arms and mining tools were made of bronze.

3. χαλκος and gold among the Egyptians were first made use of at Thebes, in weapons for destroying wild beasts, and in agricultural implements.* Hyginus, indeed, expressly affirms that Cadmus, the builder of Thebes, discovered *æs* at that place; † and Pliny, that he found mines of gold on Mount Pangæus, and the method of smelting it.‡ We have seen that under the first kings of Egypt, gold mines were worked with tools of χαλκος, on account of the scarcity of iron. In the table of Isis, some of the sceptres or spears have heads which very much resemble our bronze Celts in shape.§ But bronze armour was *entirely* out of use in Egypt in the time of Psammitichus, 670 years before Christ.

4. Weapons of bronze were *partly* in use in Palæstine, in the time of David, as I have shown in the account of the armour of Goliah, and of his descendant Ishbi-benob. In Greece, about the same age, they were general, as the extracts I have given out of Homer and Hesiod decidedly prove. Even the rasp with which the cheese was grated into the cup of wine which Nestor gave to Patroclus, was of that metal.|| Seven centuries before Christ, arms of bronze were worn by the Carians and Ionians; and when Herodotus wrote his history, the Massagetæ made their battle axes, and the heads of their spears and arrows of bronze; but all sorts of weapons and tools of that metal, were looked upon as antiquities in the days of Agatharcides and Pausanias; excepting in things which pertained to religious matters, in which bronze implements were employed in the heathen temples long after the Christian era.

5. That the ancient inhabitants of Italy, in common with the

* Diod. Sic. Re. Antiq. i. 2.—In the early history of Egypt, gold appears to have been applied to the most common purposes. Many of their temples were almost wholly covered with it. A similar profusion of silver was found among the Spaniards, when the Phœnicians first visited Tartessus; and a state of society very much resembling that of the Egyptians in the time of Isis and Osiris (i. e. about 1740 years before Christ) prevailed in Mexico and Peru, when they were first discovered, with respect to gold and silver, the use of bronze tools and weapons, the state of statuary, and especially in the use of hieroglyphics.

† Fab. 247.

‡ Lib. vii. 56.

§ See Pignorius' Mens. Isiacæ Expositio, fol. 11, &c. Ed. Venet. 1605.

|| Il. xi. 639.

people of Greece, Egypt, &c. did, at some period of their history, make their edge-tools of bronze, is sufficiently plain from the use they made of them in religious matters, and from their being frequently found in the ruins of their most ancient cities: but they were fallen into disuse in the reign of Porsenna, 500 years before Christ.* And it is probable that the nations on the western side of Europe, long before the commencement of the Christian era, had begun to disuse brass in arms, because we know that in the time of Caius Marius, the Cimbrian cavalry wore steel cuirasses; and that the people of Gaul, Spain, and Britain, were acquainted with the art of manufacturing iron in Cæsar's time.

6. The era in which edge-tools of bronze were in use in Britain, cannot perhaps be ascertained with any degree of certainty. There can be no reason to suppose that iron was introduced here while bronze was used in Greece: or that the Germans should be acquainted with it before the Britons. But when iron became plentiful amongst the Greeks, as it unquestionably was in the time of Lycurgus, 900 years before Christ, it would certainly be cheaper amongst the Phœnicians than either copper or tin: if, therefore, they traded to Britain at that time, it would be their interest to barter steel for the goods they came for; and that of the Britons to receive it for edge-tools, in preference to copper. The disuse of bronze tools, and the introduction of iron ones into this country, was probably gradual. But from the above reasons, I would conclude that bronze began to give way to iron here, nearly as soon as it did in Greece; and, consequently, that all the Celts, spear-heads, swords, &c. found in our island, belong to an æra 500, or at least 400 years before the time of Christ, for iron then seems to have been general among all the people along the shores of the Mediterranean Sea.

7. The circumstance of implements similar to our Celts having been found in Herculaneum, merely proves that the scite of that city was once tenanted by men ignorant of the use of iron; and we know from Dionysius Halicarnassensis, that it was founded about thirty years before the Trojan war. Also the various culinary and kitchen implements of bronze that abound in its ruins, prove nothing more than that the ancients had discovered that in warm climates copper or bronze is better adapted

* Since this paper was written, I have found a reference to bronze weapons in Pliny. Speaking of the medicinal qualities of iron, he says:—"Est et rubigo ipsa in remediis: et sic Telephum proditur sanasse Achilles, sive id æreа, sive ferrea cuspide fecit. Ita certe pingitur dicutiens eam gladio." He doubted whether this healing rust was scraped off a bronze or an iron sword, because he knew that in the heroic age, bronze was in use in weapons. He could have had no difficulty in concluding that it was not of bronze, from any use to which that metal was applied in arms in his time; for his own accounts of iron sufficiently refute such a notion; and in the chapter from which this extract is taken, he says:—"Medecina è ferro est et alia, quam secandi," from which it is plain that surgical instruments were made of it in his time.—Nat. Hist. xxxiv. 15. Hygin. 101. Paus. Arc. lxv. 4. Ovid. Metam. xiii. 172. Trist. v. 2, 15. Remed. Am. 47, &c.

for such purposes than iron. I apprehend too, that nothing more can be inferred from the fact, that both Celts and undoubted Roman antiquities have been met with at Ladbrook, in the middle of the town of Old Flint, than that the Britons had occupied that situation either as a fortress or a town before the Romans settled in it.

8. That the Celts were not imported into Britain is plain, from moulds for casting them in, and pieces of crude bronze being found in places where, from the cinders that were with them, they appeared to have been cast. If the bronze of which they made them was imported, it is probable that the people who supplied them with it exchanged it for tin, one of the articles of which it was composed. But it cannot be supposed that a people, whose country abounded with copper, should be ignorant of the art of working and smelting it, at a time when they were mining and manufacturing tin, lead, and iron. The æs, which Cæsar says they imported, and the *χαλκωματα,* which Strabo mentions, were probably nothing more than vessels of copper or bronze, which foreign merchants bartered among them for hides and metals.

9. It has been shown that the sceptre or rod of Moses, and many of the utensils of the tabernacle of the Hebrews, were of brass; but none of them of iron. The Greeks and Romans borrowed a great part of their religious worship out of Egypt, where it is probable bronze, as the first metal which assisted in the arts of civilized life, was held in religious veneration; and iron, as a more modern discovery, in religious abhorrence. We accordingly find in Hesiod, that iron was prohibited in certain religious rites; and Accennius, on the word "ahenis" in the following lines from the Æneid,

> "Falcibus et messæ ad lunam quæruntur ahenis
> "Pubentes herbæ, nigri cum lacte veneni,"

says: "Quia nefas id ferreis facere." Does not this custom justify the supposition that the "aurea falx," with which Pliny says the Druids, at certain seasons, cut the misletoe, is an error for "ærea falx?" and, consequently, that bronze implements were antiquated in his time in all common uses in Britain, and only employed in the religious rites of the Druids?

10. The extracts, I have given out of Homer and Aristotle, prove that the Phœnicians were in the habit of bartering their toys and baubles for valuable commodities in Greece and Spain; I would, therefore, infer, that they exchanged trifles of that sort amongst the Britons for tin; and, consequently, that the articles of jewelry, found in our most ancient tombs, are of Phœnician manufacture.

Article III.

A Method of fixing Particles on the Sappare. By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Oct.* 24, 1823.

When the species of minerals are ascertained by their physical qualities, they mostly undergo no injury, or but a very slight one; as that attending the determination of their hardness, the colour of their powder, their taste, &c. This is certainly a material advantage, and would highly recommend this method, was it constantly adequate to its purpose. That it is not so, however, we have a proof in the great errors into which have fallen those best skilled in it. Mr. Werner, its principal and most distinguished professor, was unable by its means to discover the identity of the jargon and the hyacinth; of the corundum and the sapphire; of his apatite and his spargelstein; and while he thus parted beings, as it were, from themselves, he forced others together which had nothing in common.

The chemical method justly boasts its certainty; but it carries destruction with it, and often bestows the knowledge of an object only at the expense of its existence. The sole remedy which can be opposed to this defect is to reduce the scale of operating; and thus render the sacrifice which must be made as small as it is possible.

M. de Saussure's* ingenious contrivance for subjecting the most minute portions of matters to fire, by fixing them on a splinter of sappare, appeared to fulfil the conditions of this problem, and to have accomplished all that could be desired. It has, however, been scarcely at all employed, owing to the excessive difficulty in general of making the particles adhere; and in consequence the almost unpossessed degree of patience required for, and time consumed by, nearly interminable failures.

That such should be the case could not but be a subject of much regret, for besides economy of matter, of time, of labour, and the great beauty of deriving knowledge from so diminutive a source, and attaining important results with such feeble agents; reduction of volume became, in this instance, productive of increase of power, and thence, of an extension of the series of qualities by which substances are characterised.

A slight alteration which I have made in M. de Saussure's process has removed the objection to it. To water, saliva, gum water, which he employed, the last of which is not sensibly

* Journal de Physique, par Rozier, tome 45.

superior to the former, I have substituted a mixture of water and refractory clay.

Small triangles, or slender strips, of baked clay may be used in lieu of sappare, which is not at all times to be procured; or a little of the moist clay may be taken up on the end of a platina, or other wire, and the object to be tried touched with it. This way may be applied to pieces of the ordinary size, and supersede the use of the platina tongs.

But a proceeding which I have only recently adopted appears to deserve the preference. Almost the least quantity of clay and water is put on the *very end* of a platina wire, filed flat there. With this, the particle of mineral lying on the table can be touched in any part chosen; for a moment or two it is dry, and may be taken up, and put into the flame, without the clay exploding, as not unoften happens when more of it is used. Particles of the least visible minuteness may be thus submitted to trial with the utmost facility. The contact of the particle with the wire may, in general, be so managed as to be extremely slight, as the slenderest point is sufficient to support it. However, when the utmost heat possible is desired, a fragment of a less conducting matter may, if deemed necessary, be interposed.

There may be cases in which the presence of the clay is objectionable. I conceived that some of the body itself to be tried, would, on these occasions, supply its place. Flint was the least promising of any in this respect. It was selected for the experiment. With a paste of its powder and water, pieces of flint were successfully cemented to flint, and some of this paste taken on the end of a wire, served, if not quite as well as clay, yet very sufficiently. After several times igniting and quenching in cold water, the reduction of very hard matters to subtile powder is attended with no difficulty.

Earth of alum would perhaps be preferable to pipe-clay for making the triangles on strips, and for agglutinating objects to them. It would even have the advantage over sappare of being a simple substance. Some from the Paris shops acquired only little solidity in the fire; but I afterwards learned that it had been obtained from alum by fire.

Since I have been in possession of this means of so effectually confining the subjects of examination as to be able to continue during pleasure to act on them, I have directed but little attention to the fusibility of matters. Quartz, whose fusion has been called in question by M. Berzelius,* has seemed to be quite refractory. On some few occasions when it has proved otherwise, the phenomena have neither corresponded with M. de Saussure's account, nor been always the same, which

* De l'emploi du Chalumeau, p. 108.

certainly admits of the fusion being attributed to an accidental cause.

But I have found with much surprise that flint can be melted without difficulty; and even of a considerable bulk. Where the heat is most intense, a degree of frothing takes place; where it is less, there is a swelling of parts of the surface. The effects were the same with French and English flint, with black and with horn-coloured. Does flint, like pitchstone, contain bitumen, which, at a certain heat, tends to tumefy it? This might explain the smell from its collision, and the oil which Neumann obtained by its distillation, and to which no credit has been ever given. No doubt can, I conceive, be entertained of flint being a volcanic production. On this point I may speak again at a future opportunity.

In using mere water, diamond, anthracite, plumbago, were particularly difficult of trial, as any adhesion they had contracted with the sappare was quickly destroyed by the combustion of their surface, while, as the intention in their case is not to subject to great heat, they may be so secured in the clay as at least very much to retard their escape. Here acting on very minute particles is essential, as when large pieces are employed, the effect is too slow to be perceptible.

A pleasing way of demonstrating the combustion of plumbago, and of even exhibiting the iron in it, is to rub a little from the wetted point of a pencil on one of the clay plates mentioned in a former paper.*

In trying diamond it was imagined that its glow continued an unusual time after removal from the fire. The present method afforded the means of making a comparison. A fragment of diamond, and another of quartz, chosen purposely of rather a larger size, were fixed near each other in the clay; and it was observed that the diamond was most luminous while under the action of the flame, and longer so after removal from it. Its being a very slow conductor of heat may occasion in part the latter quality.

In the same way the unequal fusibility of two substances may probably, on some occasions, be ascertained; and serve, from deficiency of a better, as a means of distinction between them.

I am, Sir, yours, &c. J. SMITHSON.

* *Annals* for May.

Article IV.

On the Ratio of Expansion of Gases. By Mr. Matthew Biggs.

(To the Editor of the *Annals of Philosophy.*)

SIR, *63, Great Russell-street, Nov. 3, 1823.*

Having had occasion lately to turn my attention to the nature of gaseous bodies generally, and particularly to their conduct under varying temperature, I referred to the works of Dr. Henry and Mr. Brande for information. We are told by both these gentlemen that all aeriform bodies possess the same mechanical properties; that the rate of expansion and contraction under increased or diminished temperature is common to all, and that, according to the experiments of M. Gay-Lussac, which they consider as the most correct, the expansion on increase of temperature is $\frac{1}{480}$ of the volume for every degree of Fahrenheit's scale, between 32° and 212°. I then proceeded to the rules which are given for reducing any volume at any temperature to such other temperature as may be required, and I found them so defective that I doubt not I shall prove to you, that all calculations made from the data there laid down, must have produced erroneous results.

After having informed us as above that the increase is $\frac{1}{480}$ of the volume for every degree of the thermometer, they proceed to say, that in order to reduce any given volume at any known temperature to any other that may be required, we must divide the whole volume by 480, multiply the quotient by the number of degrees between that at which the gas is, and that to which it is to be reduced, and then add this product to the volume, if the reduction be made from a lower to a higher temperature; subtract it if from a higher to a lower; the number now found will be the volume at the temperature required. Thus I have 100 cubic inches of gas at 32°; and my object is to ascertain what space they would occupy at 60°,

$$100 \div 480 = \cdot 208\,;\ \cdot 208 \times 28 = 5 \cdot 824\,;\ 5 \cdot 824 + 100 = 105 \cdot 824$$

they will have become 105·824 C. I. by an elevation of 28°; but suppose the reverse to be the state of the inquiry, having 105·824 C. I. at 60°, I wish to know their volume at 32°,

$$105 \cdot 824 \div 480 = \cdot 220\,;\ \cdot 220 \times 28 = 6 \cdot 16\,;\ 105 \cdot 824 - 6 \cdot 16 = 99 \cdot 664.$$

This cannot be correct, because we know that although bodies expand by the application of heat, they regain their former dimensions when reduced to their former temperature. If I find that 100 C. I. of any gaseous body become 105·824 by an addition of 28° to their temperature, I know that by abstracting the 28° they are again reduced to 100; but not lower, as this mode of calculation would show.

To put the error in another light; it is proved that 480 C. I. at 32° become 508 if elevated to 60°. The volume I set out with increases one cubic inch for every additional degree. Now suppose the temperature raised to 61°, the volume will then be 509 C. I. or it will have increased one inch; but 1 is not the $\frac{1}{480}$ of 508; therefore gas does not increase $\frac{1}{480}$ of its volume for each degree, at any point of the scale, except 32°, as the works I have referred to inform us: 100 volumes at 50° do not become 102·08 if raised to 60°, neither do 100 at 70° become reduced to 97·92 if lowered to 60°, as the examples there given seem to show.

Taking the fact that 480 volumes at 32° increase one volume for every additional degree as a foundation, we may easily form a rule by which to ascertain what space any volume at any temperature will occupy at any other temperature between 32° and 212°.

Add the number of degrees which the gas is above 32°, to 480; this will be the first number.

Add the number of degrees which the required temperature is above 32°, to 480 for the second number.

The volume on which the calculation is made will be the third, and the fourth will be the volume required.

For example, I have 100 C. I. of gas at 70°, and I wish to know what their volume would be at 60°,

$$480 + 38 = 518 \text{ first number,}$$
$$480 + 28 = 508 \text{ second number;}$$
$$\text{then } 518 : 508 :: 100 : 98{\cdot}069$$

o rsuppose the 100 C. I. to be at 50°, what will they be at 60°?

$$480 + 18 = 498,$$
$$480 + 28 = 508;$$
$$\text{then } 498 : 508 :: 100 : 102{\cdot}008$$

I will now give an extreme case, worked both ways, to show the great inaccuracy of the old method, and the correctness of mine.

Raise 100 C. I. from 32° to 212°.

$$100 \div 480 = {\cdot}208333;\ {\cdot}208333 \times 180 = 37{\cdot}4999;\ 37{\cdot}4999 + 100 = 137{\cdot}4999$$

$$480 : 660 :: 100 : 137{\cdot}5.$$

Reduce 137·5 C. I. from 212° to 32°.

$$137{\cdot}5 \div 480 = {\cdot}286458 \times 180 = 51{\cdot}5624;\ 137{\cdot}5 - 51{\cdot}5624 = 85{\cdot}9376$$

$$660 : 480 :: 137{\cdot}5 : 100.$$

Thus the error, by the old rule, amounts to more than 14 cubic inches. I am, Sir, yours, &c.

Matthew Biggs.

Article V.

A Description of some Insects which appear to exemplify Mr. William S. Macleay's Doctrine of Affinity and Analogy. By the Rev. William Kirby, MA. FRS. and LS.*

No objects are more interesting to the scientific naturalist than those which assume the external appearance of one tribe, while their more essential characters and their habits indicate that they belong to another. These objects a *prima facie* survey would often induce us to refer to a very different set of beings from that to which a more intimate acquaintance with their peculiar diagnostics and economy would lead us. And we shall find, the further we extend our researches, the traces of that plan of Creative Wisdom by which a *symbolical relationship*, if I may so call it, connects such of his creatures, as in other respects are placed in opposition to each other, as well as a *natural affinity* those that really approximate. Writers in every department of natural history, when they have been endeavouring to thread the labyrinth of affinities, have been extremely puzzled by this remarkable circumstance. They were aware that those species which connect two proximate tribes, generally partake of the characters of both ; but they were not sufficiently aware of this resemblance between objects that are connected by little or no affinity. Hence it has happened not unfrequently, that objects have been referred not to the tribe to which they are really *related*, but to that which they resemble in some of their less essential characters.

Mr. W. S. Macleay, in his acute and learned *Horæ Entomologicæ,* has furnished the naturalist with a clue which, if heedfully followed, will enable him to guide himself through all the intricacies with which the circumstance here mentioned has perplexed his path. This gentleman has first stated with clearness and precision the distinctions, so often before confounded, between real affinity and those resemblances which are merely analogical; and has proved satisfactorily, that there exist between numerous objects in every department of nature striking coincidences as to external characters, which do not indicate that they are related to each other, or should be placed together in a natural arrangement.

In confirmation of the doctrine here alluded to, I have the honour to present to the Linnean Society a description of three new genera of insects which appear to wear the face of a tribe to which they do not belong.

* From the Linnean Transactions for 1823, Part I.

COLEOPTERA Pentamera.

(*Harpalidæ.*)

Catascopus *Kirby*.

Character Essentialis.—*Labium* s. *Ligula* tripartitum: lobo intermedio abbreviato; lateralibus apice latioribus. *Labrum* emarginatum.

Character Artificialis.—*Labium* tripartitum: lobo intermedio abbreviato. *Labrum* emarginatum: lobis rotundatis. *Palpi maxillares* articulo secundo incrassato. *Oculi* magni, valde prominuli. *Tibiæ anticæ* intus medio emarginatæ.

Character Naturalis.—Corpus subdepressum, oblongum, glabrum, alatum. Caput horizontale, subtriangulare: collo distincto. *Labrum* subquadratum, apice emarginatum: lobis rotundatis. *Mandibulæ* subtriquetro-trigonæ, apice forcipatæ incurvæ acutæ, edentulæ. *Maxillæ* lobo interiori incurvo unguiformi acutissimo, exteriori palpiformi biarticulato: articulis longitudine æqualibus. *Palpi maxillares* quadriarticulati: articulo primo minutissimo; secundo reliquis longiori crassiori subarcuato; tertio secundo breviori subclavato: extimo teretiusculo. *Labium* tripartitum: lobis coadunatis; lateralibus longioribus dilatatis semicordatis planis: intermedio lateralibus dimidio breviori convexo apice bisetigero. *Palpi labiales* triarticulati? *Mentum* trilobum: lobo intermedio brevissimo rotundato. *Antennæ* undecim-articulatæ subfiliformes: articulo primo incrassato; proximis tribus subclavatis sequentibus tenuioribus; reliquis oblongis compressis; extimo acuto. *Oculi* laterales, magni, valde prominuli. *Frons* apud oculos longitudinaliter bistriatus. *Nasus* s. *Clypeus* transversus: apice segmento circuli dempto.

Truncus. *Thorax* quadrato-obcordatus, postice constrictus: angulis prominentibus, in medio canaliculatus, apud angulos posticos foveatus, antice et postice truncatus: lateribus marginatis. *Prosternum* lineare, apud basin pedum anticorum deflexum, apice rotundatum. *Mesosternum* brevissimum, apice emarginatum. *Metasternum* antice et postice mucronatum. *Scutellum* minutum, triangulare. *Elytra* apice oblique præmorso-truncata. *Epipleura** linearis, apud basin elytri dilatata. *Tibiæ* calcaribus 2. 2. 2. anticæ intus ante medium emarginatæ. *Tarsi* articulo penultimo integro.

Abdomen in specimine nostro mutilatum.

At first sight the little insect exhibiting these characters might be mistaken for a species of *Notiophilus* of Duméril, or at least be regarded as belonging to a cognate genus. Its large and very prominent eyes, the shape in some measure of its tho-

* See this term explained *Linn. Trans.* xii. 377.

rax, the striæ of the disk of its elytra less impressed with puncta than those of the limb, as likewise its frontal furrows, give it no inconsiderable appearance of affinity to it. But a closer inspection proves that this is merely an *appearance,* and that in fact it belongs to a different tribe connected with the *Harpalidæ. Notiophilus* and its genuine affinities are distinguished by a particular character indicating some difference in their mode of taking or retaining their prey. The great majority of the *Carabi* of Linné are remarkable for a notch on the inner side of their anterior tibiæ, armed at its upper angle by a spur, which appears to be of use to them for the above purpose. In the *Harpalidæ* and many others this notch is nearly in the *middle* of the tibia; but in *Notiophilus* and its affinities its situation is close to its apex. *Catascopus,* with respect to this part, falls into the former tribe. Again, in *Notiophilus* the labium consists only of a single lobe, or at least the lateral ones are much shorter than the central; but in the *Harpalidæ* they are as long or longer.* In *Catascopus* also they are very conspicuous, being twice the length of the central lobe. In *Elaphrus, Notiophilus, Blethisus,* &c. the fore-breast (*antepectus*), or the part immediately under the thorax, is more or less covered with impressed puncta. In the *Harpalidæ* and *Catascopus* it is quite free from them. In the former tribe likewise the legs, especially the thighs, are slenderer and less robust than in the latter. The head moreover in *these* is narrower behind, so as to form a distinct neck; whereas in *those,* if any thing, it is widest behind, and the neck is formed by the convexity of that part, and not by any constriction of it. From all these circumstances, I think, it is sufficiently evident, that the relation of *Catascopus* to the *Harpalidæ* is that of *affinity,* while that which it bears to the *Elaphridæ,* insects which at first sight it most resembles, is merely that of *analogy.* But there is still another tribe of which it exhibits many characters, I mean those which constitute M. Latreille's first section of his *Carabici,* which have the head and thorax much narrower than the abdomen, and truncated or very obtuse elytra; for instance, *Anthia, Brachinus, Lebia,* &c.; and with these at one time I felt inclined to arrange the genus I am considering; but the different characters of the *Labium* convinced me that it ought rather to go with the *Harpalidæ.* Should any master in Entomology hereafter undertake a new arrangement of *Carabus* L., he may perhaps bring the *Harpalidæ* and the above section nearer to each other; and in this case *Catascopus* would very well connect the two tribes. The exact place of the genus I have not been able satisfactorily to ascertain. Of all the known genera of the *Harpalidæ* it seems to approach nearest to *Pterostichus* Bon., or *Sphodrus* Clairv., principally on account of the shape of the thorax; but there must be several intermediate links between them.

* Clairville, *Ent. Helvet.* ii. t. x, xi, xii, &c. c.

Hardwickii. 1. C. Long. corp. lin. $4\frac{1}{4}$.

Habitat in India a D. Hardwicke lectus? Ex Mus. D. Marsham.

Corpus nitidum nigrum, supra violaceo et viridi tinctum. *Labrum* infra apicem utrinque punctis duobus impressis setigeris. *Frons* antice in medio canaliculatum. *Elytra* sublacunosa striata: striis, præcipue lateralibus, punctatis. Puncta insuper tria impressa inter striam a sutura secundam et tertiam. *Elytri* latera viridi-æneа.

The individual specimen here described being transfixed by the same peculiar pin which Major-General Hardwicke used for all the small insects that he collected in India (many of which he gave to the late Mr. Marsham, at whose sale I purchased it), I think I am warranted in my conjecture that this was one of them. I have therefore named it after this indefatigable collector and observer of insects, who merits richly to be so distinguished. There are two or three species apparently belonging to this genus in the rich collection of insects brought by Dr. Horsfield from Java.

(*Scolytidæ?*)

Pseudomorpha, *Kirby.*

Character Essentialis.—Labium apice tridentatum. *Palpi maxillares* breves cylindrici.

Character Artificialis.—Labium apice tridentatum: dentibus æqualibus, rotundatis. *Labrum* transversum, integrum, apice rotundatum. *Palpi labiales* articulo extimo maximo, securiformi. *Palpi maxillares* maxilla haud longiores, cylindrici. *Antennæ* breves. *Caput* transversum sessile.

Character Naturalis.—Corpus depressum, oblongum, alatum. Caput transversum, subrhomboidale, leviter inclinatum, thoracis sinu receptum, sessile. *Labrum* transversum, apice rotundatum. *Mandibulæ* forcipatæ, breves, subtriquetro-trigonæ, apice edentulæ acutæ, basi intus in lobum rotundatum dilatatæ. *Maxillæ* breves: lobo interiori incurvo unguiformi acutissimo, intus setis ciliato; exteriori palpiformi biarticulato lobo interiori arctissime incumbenti. *Palpi maxillares* maxilla vix longiores, incrassati, cylindrici, quadriarticulati: articulis brevibus; primo reliquis minori obconico, sequentibus duobus cylindricis æqualibus, extimo paulo longiori apice truncato. *Labium* minutum, brevissimum, apice tridentatum vel subtrilobum: lobis rotundatis; intermedio setis duabus instructo. *Palpi labiales* securiformes triarticulati: articulo primo brevissimo; secundo paulo majori subtriangulari; extimo maximo fere trapeziformi. *Mentum* trilobum: lobis subæqualibus, acutis. *Antennæ* capite longiores, undecim-articulatæ, filiformes: articulo primo incrassato arcuato; secundo sequentibus breviori apice incrassato; reliquis longitudine fere æqualibus, oblongiusculis, extimo acuto. *Oculi* late-

rales, minus prominentes, subrotundi. *Nasus* declivis, apice transversus.

Truncus. *Thorax* transversus, antice angustior, sinu lato pro receptione capitis exciso; lateribus rotundatis marginatis: margine explanato recurvo; angulis anticis et posticis rotundatis. *Prosternum* et *mesosternum* linearia. *Metasternum* antice et postice mucronatum. *Scutellum* triangulare. *Elytra* oblonga latere exteriori marginata: margine subrecurvo, apice obtusissima, vel oblique subtruncata: epipleura lineari apud basin elytri dilatata. *Pedes* breves: femoribus magnis compressis; tenuioribus; tibiis calcaribus 2. 2. 2.; anticis intus ante medium emarginatis: tarsis subsetaceis; articulo penultimo integro: unguiculis binis simplicibus.

Abdomen depressum: segmentis ventralibus sex; tertio reliquis longiori; anali obtusissimo.

Catascopus merely assumes the aspect of a section different from that to which it really belongs, while every one sees at first sight that it is one of the *Carabi* of Linné; but the insect I have now described, though it exhibits the *characters*, has not the *aspect*, of that tribe; and even a practical entomologist, if he chanced to examine a specimen that had lost its antennæ, might at first regard it as a *Nitidula* or *Ips* F., or as coming near that genus in the system. But when he came to study it in detail, he would discover, to his surprise, all the essential diagnostics of one of Latreille's *Entomophagi*, as six palpi,* and the trochanter forming a fulcrum to the posterior thigh; and further, those that distinguish the *Carabici* of that author, the same kind of *labium*, *mentum*, and *maxillæ*, and particularly the remarkable notch in the inside of the anterior tibia, before noticed, peculiar to them. The characters that give it an air and general appearance unlike those of its tribe, are its sessile wide head received into the thorax, and its short antennæ and legs.

It is difficult to say to which of Latreille's sections of his *Carabici* it bears the greatest affinity. Its depressed body, its elytra very obtuse at the tip or subtruncate with an *epipleura* dilated at the base, and its blunt anus, seem to indicate an approximation to *Lebia*, *Dromius*, &c. and the labial palpi are not unlike those of one sex in *Tarus* Clairv. (*Cymindis* Latr.) belonging to the same section; but its sessile head brings it nearer to *Scolytus* Fab. the labium of which is not very dissimilar, and to the aquatic *Entomophagi*. Its thorax is shaped very much like that of *Hydrophilus caraboides*. Its maxillary palpi are unlike those of any other entomophagous genus yet known. Many links, however, remain to be discovered before we can connect this remarkable and puzzling genus with any one at

* What has been accounted by Fabricius and others as an additional or inner maxillary palpus is, strictly speaking, the outer or upper lobe of the maxillæ become palpiform. In *Staphylinus*, &c. this lobe is also *biarticulate* but not *palpiform*.

present known. In going over most of the cabinets in London, I could discover nothing that came at all near this insect, which I purchased at the sale of the late Mr. Francillon's collection. From the mode in which it is transfixed, and the pin used, I suspect that it was taken by Mr. Abbott in Georgia.

excrucians. 1. P. Long. corp. lin. 5.

Habitat in Georgiæ forsan aquaticis? Ex Mus. D. Francillon.

Corpus læve, nitidum, subpilosum, rufum. *Labrum* antice punctis quatuor excavatis setigeris. *Oculi* in medio pilosi. *Coleoptra* seriatim subpunctata, picea: margine externo rufo.

(*Melolonthidæ.*)

Mimela, *Kirby*.

Character Essentialis.—*Mandibulæ* dorso rotundatæ, apice compressæ bidentatæ: dente inferiori truncato. *Antennæ* novem-articulatæ.

Character Artificialis.—*Labium* urceolatum, emarginatum. *Maxillæ* apice sex-dentatæ, nempe 3. 2. 1. *Mandibulæ* dorso rotundatæ, apice compressæ bidentatæ: dente inferiori truncato. *Labrum* brevissimum, transversum, medio depresso-excavatum, vel emarginatum. *Antennæ* novem-articulatæ. *Podex* tectus.

Character Naturalis.—Corpus ex oblongo obovatum, convexum, glabrum, alatum. Caput ex triangulari subrotundum, declive. *Labrum* transversum, brevissimum, medio depressum, utrinque antice barbatum, verticale. *Mandibulæ* basi subtriquetro-trigonæ, intus orbiculatæ transversim sulcatæ, apice compressæ incurvæ bidentatæ: dente superiori obtuso, inferiori truncato subemarginato, dorso rotundato. *Maxillæ* validæ mandibulæformes, apice incurvæ sex-dentatæ, dentibus nempe 3. 2. 1. *Palpi maxillares* in nostris speciminibus desunt. *Labium* infra apicem et apud basin constrictum unde quasi urceolatum, apice emarginatum. *Palpi labiales* triarticulati: articulo primo minutissimo, intermedio subarcuato crassiori; extimo ovato acuto. *Mentum* subquadratum. *Antennæ* novem-articulatæ: articulo primo magno apice incrassato, quasi dolabriformi; secundo brevi subturbinato; proximis tribus subcylindricis; sexto brevissimo fere pateræformi; tribus ultimis elongatis pilosis, clavam elongatam linearilanceolatam formantibus. *Oculi* subhemisphærici prominuli. *Septum* irregulare a naso per tertiam fere partem oculi transcurrit. *Nasus* s. *clypeus* transversus, distinctus, antice rotundatus, marginatus: margine reflexo. *Rhinarium* verticale, brevissimum.*

Truncus. *Thorax* transversus s. longitudine latior, tenuissime

* I call the part often conspicuous in this tribe, intervening between the nasus and the labrum, *Rhinarium*.

marginatus, antice angustior; sinu magno ad recipiendum caput exciso, postice obsolete trilobus: lobo intermedio rotundato, supra ad latera puncto ordinario impressus. *Prosternum* inter pedes anticos elevatum, compressum, apice dilatatum, oblique truncatum. *Mesosternum* lineare, inter pedes intermedios latitans. *Metasternum* basi et apice mucronatum: mucrone postico bifido. *Scutellum* triangulare. *Coleoptra* oblonga, striata: striis duplicatis, podicem, excepto summo vertice, obtegentia. *Pedes* robusti: femoribus posticis incrassatis; tibiis anticis apice bidentatis: dente exteriori longiori obtuso; interiori brevi acuto; calcaribus 1. 2. 2. posticis obtusis; tarsorum unguiculis simplicibus inflexis.

Abdomen convexum: segmentis ventralibus sex; primo brevissimo; ultimo depresso obtuso.

The insect from which I have taken the characters of this genus I originally met with at a dealer's; and though it was transfixed with a needle, which seemed to indicate that it was from China; yet as his insects were almost all of them Brazilian, and its general habit and aspect were that of a tropical American type, I concluded that it came from that country, and placed it in my cabinet along with my species of *Areoda* of Macleay. Afterwards, being shown by a young lady a collection of undoubted Chinese insects, I found amongst them several specimens of *Mimela*, one of which she kindly gave me. Upon receiving this, on my return to Barham I set about a closer examination; and upon dissection I found, though many of its external characters seemed borrowed from South American types, yet that in those which were most essential, it came nearest to an Asiatic one, a well known species of which was abundant in China; and others have since been discovered in Java, and perhaps in Ceylon. I allude to Mr. W. S. Macleay's genus *Euchlora*.

The Brazil genus, of which *Mimela* assumes the external appearance, is *Areoda* of the same learned author, who has observed with regard to *Euchlora*, "En genus Asiaticum *Areodæ* proximum!"* But that which I am describing still more nearly resembles it, wearing, as it were, its very habit; so much so, that at first sight it might almost be mistaken for a small specimen of *Areoda Leachii*. The general colour of the animal; the sculpture of the head, thorax and elytra; its distinct nasus or clypeus; its labium, labrum, maxillæ and legs, are all very similar. But in *Mimela*, as in *Euchlora*, the mandibulæ are concealed under the nasus; whereas in *Areoda* they are very visible, nor have they the dorsal process or tooth observable in the *Rutelidæ*. In the two former, the antennæ consist of nine joints, in the latter of ten. In *them* the posterior lobe of the thorax is more obsolete than in *this*. In *Areoda* the last dorsal

* *Horæ Entomolog.* 148.

segment of the abdomen is not covered by the elytra; but in *Mimela* (a circumstance in which it agrees with *Pelidnota* Macleay, another South American type), only the tip is uncovered. The latter, *Mimela*, has an elevated prosternum, and a metasternum with a very short anterior mucro, so as to leave the mesosternum visible; whereas in the former, *Areoda*, the prosternum is not visible without dissection, and the anterior mucro of the metasternum is elongated so as entirely to cover and conceal the mesosternum. The abdomen also in *Areoda* is covered underneath with an infinity of very minute punctula, which give it a silky appearance; whereas in *Mimela*, and likewise *Pelidnota*, it is lævigated.

Though *Mimela* agrees in most of its essential characters with *Euchlora*, it differs sufficiently to form at least a *subgenus* in a modern system. In the former the mandibulæ have only two teeth at their apex; in the latter they have three. In *this* also the body is covered with innumerable impressed puncta of the same size; whereas in *that* the puncta are of two sizes, the larger scattered, the smaller almost invisible and quite covering the surface. In *Euchlora* the last dorsal segment of the abdomen and part of the last but one are uncovered, the very reverse of which, as we have seen, takes place in *Mimela*. Whether the inner claw of the four anterior legs is bifid at the apex in the latter as it is in the former I cannot say, these tarsi being mutilated in my specimens.

I shall here mention one very remarkable circumstance, noticed by no writer that I have met with, which distinguishes the mandibulæ of the tribes of *Melolontha* F., though less conspicuous in *Melolontha* itself than in the *Euclora*, *Rutelidæ*, *Anoplognathidæ*, *Chalepus*, &c. The *molary* part, or that which appears destined to comminute the food, is an orbicular or subquadrate flat plate at the inner base of the mandibles, scored out into numerous alternate transverse ridges and furrows. When the mandibles are open, the food, after it has been divided by their apex, must pass between these plates, which, supposing that the ridges of one mandible are received by the furrows of the other, as is most probably the case, must have vast force in comminuting the food, not so much by the friction of the plates, since that could scarcely take place in consequence of the above structure, but from their pressure and the action of the sharp ridges. The mandibula indeed is particularly fitted for this double office, the upper part being thin and adapted to cutting, and the base vastly thick and strong, as if its office was great pressure. At the base of the mandible in the genus before us, but not in all, there are other short furrows forming an acute angle with the transverse ones, and opening into the gullet. In the *Dynastidæ* Macleay, the molary space is visible, but is smaller, and has fewer furrows. In *Dynastes Enema* it has only two obtuse ridges, and as many furrows, and appears evidently

calculated to masticate, but more grossly, a harder substance than what is submitted to the action of the mandibles of *Melolontha* F. In a specimen of *Areoda* I found adhering to this molary plate, a substance resembling the pollen of flowers, which may hence be conjectured to be the food of that genus.*

From this account it seems I think evident, that a modification of the three kinds of teeth of vertebrate animals is to be found in these tribes as well as the *Orthoptera,* in which Marcelle de Serres detected them; for we find the *incisores* at the apex of the mandible, the *molares* at its base, and the *laniarii* at the apex of the maxillæ; though with respect to these last, I believe their primary use in very many insects is to hold the food for the action of the mandibulæ.

Mimela Chinensis.

Long. corp. lin. 9. *Habitat* in China. Ex Mus. D^næ^ Crane.

Corpus glaberrimum, luteo-virens, colore sub luce mutabili, subtus cupreo tinctum. *Caput* supra antice punctis confluentibus rugulosum, postice punctis sparsis conspersum, interque puncta creberrima minutissima, vix sine lente forti conspicua, subtus fulvum. *Antennæ* fulvæ. *Thorax* punctis sparsis punctulis minutissimis interjacentibus ut in capite. *Elytra* subrugosa, puncto-striata: striis intermediis per paria ordinatis, interstitiis punctatis et punctulatissimis ut in thorace, &c. apice gibba.

The insect I shall now describe is of a different order; and though it does not so strikingly assume the characters of another tribe or genus; yet, as it appears to partake of those of both *Agrion* and *Lestes,* exhibiting the general appearance and wings of the former, with some diagnostics of the latter, it seems not improperly introduced.

Agrion Brightwelli.

Nigrum: alis basi, in altero sexu apice macula, sanguineis. Long. corp. unc. $2\frac{1}{4}$. Expans. alar. unc. $2\frac{3}{4}$. *Habitat* in Brasilia. Ex. Mus. D. Brightwell.

Corpus nigrum, sub sole splendore obscure metallico subnitens. *Caput* subpilosum. *Truncus* brunneo-niger, supra lineis tribus longitudinalibus, intermedio elevato, nigris, sub alis primoribus strigis tribus obliquis, superiori obsoletiori, pallidis. *Alæ* subhyalinæ, basi læte sanguineæ, posticis apice macula subrotunda ejusdem coloris. *Stigma* nigrum oblongo-quadratum. *Abdomen* elongatum, tenue, transversim rugulosum, basi et apice subincrassatum. *Forceps* analis rectus? inferiori subincurvo.

* Since this paper was written, I met accidentally with a passage in Cuvier's *Anatomie Comparée* (iii. 321 —.), by which it appears that he had observed in the mandibles of the larvæ of the *Lucani* "*vers leur base, une surface molaire plane et striée;*" but he does not appear to have noticed this structure in any perfect insect.

N. B. In quibusdam speciminibus macula rotunda sanguinea alas primores item apice ornat. An sexûs varietas?

Nomen dedi in honorem D. Brightwell *Norvicensis, insectorum collectoris indefessi, felicis; indagatoris acuti, docti.*

The upper anal forceps in the specimens of this insect that I have had an opportunity of examining were mutilated; I cannot therefore be positive that it does not approach nearer to *Lestes* of Dr. Leach, the stigma of which its wings exhibit, than to *Agrion;* but as these last are not suddenly narrower at their base, as in the *former* genus, I have considered it as belonging to the *latter*.

Article VI.

Some Account of Maier's Symbola Aureæ Mensæ Duodecim Nationum. By the Rev. J. J. Conybeare, MGS.

(*Concluded from* p. 247.)

Book V. *Arabian School,* led by Avicenna.—Maier here arrives at a period when the alchemical art did really flourish. Many of the writers whom he quotes were addicted to its study, and, in all probability, composed the works which pass under their names. One may fairly, however, except the first hero of this section, Mahomet, whom, after some questioning, he determines to have been a *philosophus per ignem,* upon his favourite grounds, that the possession of those extensive pecuniary resources always necessary for leaders or monarchs, is best accounted for by the supposition that they *had the secret.* It had been well for mankind if Mahomet, and some other of Maier's alchemical warriors, had possessed no better means than the Hermetic Gold of attaining to that *bad eminence* for which we usually suspect them to have been indebted rather to the *Martial Steel.* Of Avicenna nothing very remarkable is stated. He is followed by Geber, the obscurity of whose writings is acknowledged and defended at large, chiefly on the score of the enormities which would follow if the chrysopoetic art were plainly taught, and generally practised. It never seems to have occurred to Maier that if gold were thus multiplied it would become comparatively valueless. Artephius, the next associate of Avicenna, appears to have written on metaphysics and necromancy. Maier, however, contends, that it had all an alchemical meaning. Next follows Rhazes, from one of whose *dicta* it should appear, that the alchemist's secret lay not so much in the materials with which he operated, as in their respective quantities. "*Quicunque ignorat pondera, non laboret in nostris libris; nam philosophi nihil suarum rerum posuerunt, nec aliud occulta-*

verunt, nisi pondera." A great number of Arabian philosophers are mentioned, and a story quoted from the "*Aurora Resurgens*" of a Christian captive receiving from his Saracen master his freedom, and a portion of the stone. This was capable not only of transmuting metals, but of healing. "Give the powder (says the adept) to a leper; let him go to bed, and cover himself with a counterpane, or sudary (*sudario*), and he shall be cured." It seems not improbable that some powerful mercurial, or rather antimonial remedy, was occasionally administered under this mysterious veil. Maier allows throughout this chapter that many *soi-disant* speculators in the art were no better than imposters. He professes to give some account of the points "*in quibus omnes chemici conveniunt.*" All metals they universally believe to be generated beneath the earth from *fumes* (which are hot and dry), and *vapours* which are cold and moist. The rest of his argument seems to amount to this, that these four elements uniting in different proportions to form every known metallic substance, it is possible for art so to readjust those proportions as to convert the baser metals into the more perfect. "Ask (he continues) whether iron be not converted into copper at Goslar and elsewhere,* iron into steel, and *lead into mercury.*"

Book VI. *The German School,* led by Albertus Magnus, the account of whose life contains but little interesting. Maier rejects the traditionary tales of gold found in a human scull, and the vine with golden tendrils, and the golden-toothed Silesian boy. It is singular that he does not contrive to find allegories in them. Albert is followed by Bernhard, of Treves, whose *hermetic philosophy* is "*in manibus omnium et admiratione,*" and Basil Valentine, whose works "*doctorum indoctorumque manibus quotidie terantur.*" The fame of the latter has survived that of the former.†

Valentine is followed by Alanus de Insulis, better known to English antiquaries as the Expositor of the Prophecies attributed to Merlin. A "Liber Chemiæ" is quoted as his production, and great merit is ascribed to him for applying to the purposes of chemical digestion and evaporation the heat of a dunghill. It seems to have been an object with the *artists* to obtain a continued and equable heat, lower than that of the furnace, and not supported by any visible fire. R. Lullius for this purpose used a mixture of horse-dung and quicklime. Maier is loud in the praise of this *philosophical bath,* and even minute in his directions for the choice of its principal ingredient. He shortly after dwells much on the *ignis philosophicus,* which is

* This *transmutation* obtained belief so late as a century after our author's day, even from the elder Geoffroy.—(See Le Pluche's History of the Heavens, vol. ii. p. 70.)

† See the article Antimony in most of the larger Chemical Systems. His "Currus Triumphalis Antimonii," and "Last Will and Testament," were translated into English towards the end of the 17th century.

not the same with common fire. It burns without flame, and "*in summis montibus delitescens non extinguitur.*" Elsewhere he states, that the philosophical gold, like the philosophical fire, is not the same with vulgar gold; the truest position, I apprehend, in the whole course of his work. The two Hollandi, J. Pontanus, and others, succeed, the merits of each *hero* being illustrated, as usual, by some reference to a quotation from their principal works. In two secrets ascribed to Greverus, he professes to find the stone Pantaura, and the water of gold of Philostratus. The former he states to be capable of drawing other stones to itself, and to be "*re aut certe effectu,*" the same with the eagle stone.* Of the latter, he affirms that it can be held by a metallic vessel only, and that the more compact metals are the fittest for the purpose. Maier gives a long but lame defence of that singular quack Paracelsus, and a somewhat more entertaining account of the Rosicrucians, who are formally invited to join the hermetic circle. Our author writes with the air of one extremely anxious to believe all that was reported of this mysterious fraternity, and, perhaps, to earn by his obsequiousness (like the Jew in Kenilworth) the honour of participating in their secrets. He addresses to them sundry very indifferent specimens of Latin verse. Amid the intentional obscurity and metaphysical trifling of this chapter, it is difficult to find any thing practical, or even intelligible. He mentions the increase of weight which some metals gain in the fire, as a matter altogether unaccountable "*aliquid miraculi contingit.*"

Book VII. *The French School* is led by Arnold de Villeneuve, of whose character our author enters into a long defence, rather declamatory than argumentative. The reproaches of his enemies, however, seem (as they are here represented) to have had no better foundation; they turn chiefly on his having attacked the authority of the Papal See. His claims to rank high as an alchymist must be conceded, for his contemporaries esteemed him a conjurer. So far Maier. Arnold was, however, in truth, for his age, no common man; and chemistry, as well as religion, was indebted to his researches. He is said to have discovered the spirits of wine, and of turpentine.† Arnold is followed by Vincent of Beauvais, certainly one of the most laborious and generally informed writers of the Middle Ages. His Speculum Naturale (from which Maier quotes one short sentence apparently in favour of alchemy) is the largest and most interesting

* The well-known geode containing in its hollow a moveable nodule. If this were the Pantaura, it might be among the symbols of the cosmological schools, from which we have seen that the alchemists so largely borrowed. "The genuine Pantaura is (says Maier) very scarce; but what, you will ask, has it to do with *the work?* Is not gold, I answer, generated in the hardest stones, as in pyrites, cadmia, garnet, and lapis lazuli?"

† See Arnaud de Villeneuve in the Dictionnaire Historique of L'Advocat, &c.; Mosheim Eccl. H. vol. iii. p. 36, and Flacii Testes Veritatis (sub nomine). Bergman mentions him as one of the earliest writers who notice distilled vinous spirits.

Encyclopædia which I know of the philosophy and natural history of that period. It seems to have been laid under contribution pretty largely, if not altogether copied, in a work better known to our own black letter students, "*Bartholomæus de propuetatibus rerum.*" I have now before me what a bibliographer would term a venerable and perfect copy of Vincent's S. N. (Cologne, 1494.) The sixth and seventh books contain much alchemical matter, chiefly extracted from Avicenna, and a work termed Alchemiste. From this latter, the passage quoted by Maier as Vincent's own is taken, and it occurs in the 31st chapter of the 6th book.*

Vocatur (says the alchemist of the great secret) *Elixir*, et dicitur *Lapis, non Lapis*. *Lapis* quia teritur: *Non Lapis* quia funditur et currit in igne absque evaporatione sicut aurum. *Nec est alia rescui proprietas illa conveniat*. Can he mean that there is but one substance which fulfils these two conditions *of being levigable, and fusible without evaporation?* Vincent himself is not, however, answerable for this bold assertion. He seems to have been here as elsewhere merely a transcriber and compiler of others. *Nicolas Flamel*, well known for his chemical hieroglyphics, follows; and the catalogue is terminated by the notice of some authors living in Maier's own day. One of these, Dionysius Zacharius, is vehemently defended against some nameless writer who had attacked him and his Alchemy. The defence is accompanied by a singular concession, "*that the alchemist did not succeed once in a thousand times;*" and that there was, therefore, but too much ground for the arguments by which the unskilful endeavoured to deter their friends from the pursuit of the art, and to depreciate its professors.

The next character mentioned as an alchemist affords a singular instance of Maier's blindness or deception. *Fernelius*, the physician, does not appear to have meddled with alchemy; but in a treatise "*de abditis rerum causis*," he states that having read in Hippocrates "*esse aliquid divini* (τὸθειον) *in morbis*)," he had applied himself to its discovery. Maier decides peremptorily, that the "*divini aliquid*" must be the philosopher's stone, and the *diseases* those of *metals* (as he afterwards terms lead *aurum leprosum*). He abuses accordingly those who could not penetrate, or who blamed, the obscurity of Fernel's work. He now proceeds to give a short statistical account of France (apparently from B. de la Vigenere), enlarging particularly on the revenues of the Gallican church as a proof of the riches and piety of their early kings. The book ends, as usual, with a syllogistic contest.

Book VIII. Contains the *Italian School*, headed by the certainly better known to the learned world as a theologian than an

* Maier refers for it to the 1st book, where I cannot at the moment find it.

alchemist, Thomas Aquinas. Maier, however, defends his claim to this title on the credit of some works circulated under his name. They are probably forgeries, but a quotation from one of them may serve to show how curiously the lovers of the scholastic philosophy were misled by reasoning on the Aristotelic qualities of the supposed elements. "Inveni (says the writer describing the result of his operations) quendam lapidem rubeum clarissimum, diaphanum et lucidum, et in eo conspexi omnes formas elementorum, et etiam eorum contrarietates in illa materia lapidis: Ex rubedine enim respexi formam ignis, ex diaphanitate formam aeris et ex luciditate formam aquæ." Maier, under this head, speaks more openly than usual of antimony (terram nigram oculosam, Antim. Hispanicum, Stimmi Italicum) as the chief ingredient prescribed by T. Aquinas and others for the production of the wonderful stone. From antimony one philosopher, he tells us, had produced *mercury, lead, copper, iron, silver, gold,* and *hepar.** Others (who seem to have been somewhat more honest in their professions), *Regulus, white, yellow and red flowers, oil, glass,* and *salt.* It is needless to point out to your chemical readers the probability of the latter assertion, or the modern synonymes of the substances thus obtained. He proceeds to enumerate the various places where antimony has been found; but lest he should make his treasure too common, quickly relapses into the alchemist; "*Which, you will ask, of these antimonies are we to choose? I answer, the philosophical.*" All varieties are not equally good for *the work;* and if any one fail, it is because he has chosen a wrong one, or having (as Aquinas rightly advises) worked with the Spanish, has not understood its preparation ("*e vulgari nempe faciendum est physicam.*)"

Among the followers of Aquinas, are enumerated the poets J. A. Augurellus and M. Palingenius. Their alchemical strains, if we may judge from the specimens adduced by Maier, are certainly far more classical and attractive than those of our own ancestors, for the preservation of which we are indebted to the ill-placed zeal and industry of Ashmole. Maier states that Augurellus gives wise and cautious directions for the behaviour and carriage which the adept should observe "*in order to avoid the suspicion of being an alchemist,* a suspicion which might involve him in many difficulties with the malevolent and unworthy."

Some statistical information is added; and the arguments which follow descend more to particulars than is usual with our

* It is not impossible that a practised chemist even of those early days may, in operating on large quantities of an impure grey antimonial ore, have found traces at least of lead, copper, iron, silver, and the sulphur at least of his hepar. The metallic antimony (fusible at a very low heat) might itself be one variety of the philosophical mercury.

author. He infers the possibility of fixing mercury *into gold* from the certainty of its being fixable, "fumo sulphuris ex plumbo," or "fumo plumbi crudi," (does he mean of galena?) and again from the *known fact*, that gold by the action of a certain water may be resolved into a red tincture, or colouring matter and mercury. He evidently lays much stress on the extraction of this colouring principle.

Book IX. which is meagre, and of little interest, professes to give an account of the *Spanish School*. This is led by Raymund Lully, concerning whom our author appears firmly to believe that he coined Rose nobles of alchemical gold for our Edward III. and prolonged his own life by the *Elixir* till the age of 140 years. He relates also the tale of Lully's making seven statues of the philosophical gold and silver for the church of Catalonia, which he who prefers fiction clothed in metre may see in Norton's Ordinall.* Lully was doubtless a man of various information for his day, and a most voluminous writer. He stood high in the estimation of our English alchemists. Sir E. Kelly, who denounces most of his brother adepts in a tone of true *magistery*,† says of himself,

—— "though I write not half so swete as Tully,
Yet shall you find I trace the steps of Lully."

No other Spanish *philosopher* is mentioned at any length. The chapter is eked out with some instances of Spanish cruelty, one resembling the story commonly told of Kirk. The argumentations which as usual close the book are even duller than usual; I hasten, therefore, to

Book X. which treats of the *English School*, and may be of some interest to antiquarian, if not to chemical inquirers. Roger Bacon, as might be expected, is their leader. Maier is chiefly engaged in proving him to have been no conjurer, and to have had no connexion with Friar Bungay and the brazen head. "Hæc (says he) fabulosa et fictitia sunt, quamvis in publicis comædiis populo ibi (*in Angliâ*) proponantur." The seven years' labour feigned to have been spent on this *head must have been given to the search of the stone*, which is further proved by the existence of some alchemical tracts and letters passing under Bacon's name, one of which contains a valuable chemical axiom, applicable, according to Maier, to many other works besides

* Ashmole's Theatrum Chymicum, p. 21.

† All you that faine philosophers would be,
And night and day in *Geber's* kitchen broyle,
Wasting the chipps of ancient *Hermes'* tree,
Weening to turne them to a pretious oyle;
The more you worke, the more you lose and spoile.
To you I say how learn'd so e'er you be,
Goe burne your books, and come and learne of me.
(Ashmole, T. C. B. p. 324.)

Bacon's. "*Cum dico veritatem, mendacium puta; cum mendacium, veritatem.*" To Bacon succeed *Garlandus* (so named from the title of his work), who flourished about the end of the 11th century, and made his countrymen acquainted with much of Arabian science, which he had learned during his residence in Spain. *Dastin.** Ricardus Anglus (quære if the same with the Richard Carpenter of Ashmole), from whom several extracts are given chiefly to show that the *sulphur philosophorum* is not common brimstone. *Ripley,*† of whose knowledge he speaks highly; the extracts given relate chiefly to the variety of menstrua requisite for the adept; but even here there is ambiguity; for Lully, he tells us, held that there were two only "*unum resolvens, alterum resolvendum;*" while others held three. One of Ripley's axioms (if I do not misunderstand it) bespeaks considerable practical knowledge of chemical compounds. "Omnis spiritus figitur cum calcibus sui generis;" or, as it would be expressed in the language of the present day, "Every acid forms permanent compounds with certain bases for which it has a strong affinity." *Ripley* is followed by *Norton,* whose *Ordinall,* Maier proposed to translate and publish. From this work (which may be seen in Ashmole's Theatrum Ch.) he extracts some curious narratives as to the folly of some pretenders to *art,* and the hard treatment of its real professors. He corroborates Norton's assertion as to the number and proficiency of the philosophers residing in London towards the end of the 15th century by the authority of an Englishman named Knight, who assured him (Maier) that there was still extant in the library of Westminster Abbey, a manuscript account of the sums paid to the king by these *artists.* *Cremer* (abbot of Westminster), Edward Kelly, a Scot named Willebius (Willoughby?) (whose projections had but lately astonished all Italy, France, and Germany), Giles de Vadis, Duns Scotus, and the wizard Michael Scot, make up the list of English adepts. He mentions an anecdote of an ex-monk which confirms the belief that alchemy was much studied in the conventual establishments, and the knowledge of its secrets thought to be still possessed by many of their ancient inmates.‡

Maier subjoins to this account of English philosophers what he terms a Xenium or valedictory epistle of thanks for the hospitalities he had received in this country. It contains more, however, of complaint grounded on the low estimation in which the English held all foreigners, and the illiberal manner in which they derided and insulted them, especially in their stage plays. These he describes as acted daily in four or five different

* See Ashmole, T. C. p. 257.

† See Ashmole, p. 374, &c.

‡ Ibid, p. 466, 481. and elsewhere, especially the metrical narrative of Charnock, which affords a most characteristic and interesting picture of the delusions of *the art.*

theatres, and as having a most pernicious effect on public morals. Personally he complains that they never introduce a German character but as a stammering and barbarous drunkard; and that they describe the Emperor as a petty king (Regulum). But his chief invectives are levelled against Matthew Gvvin,* who was censor of the Apothecaries' Company, but was oftener to be found in the tavern than the laboratory. Gvvin, among other *obloquies*, had affirmed that the Saxon nobles were impoverished by their pursuit of alchemy. Maier of course treats this as a villanous falsehood. He proceeds to retort the charge of national drunkenness, contending that the vice is more common, and its effects more publicly disgusting in England. He next answers at some length to Camden's assertion, that the nobles of England were more dignified and independent than those of Germany; then defends the Lutheran church, and attacks what appear to him the incongruities of our reformed worship and ecclesiastical customs. He objects especially to the touching for the king's evil, and maintains that the alledged cures were the work partly of imagination, and partly of the *alchemical power of gold*. He ends by reprobating a part of our criminal law, and our pronunciation of the Latin, and even of our own language. Maier's account of his visit to England is corroborated by Ashmole. "He came (says that eminent *mercuriophilist*) out of Germany to live in England purposely that he might so understand our English tongue as to translate Norton's Ordinall into Latin verse, which most judiciously and learnedly he did; yet (to our shame be it spoken) his entertainment was too coarse for so deserving a scholar." From the logical discussions which, as usual, close the book, we learn that it was agreed, both by the advocates and adversaries of the science; 1. *That nature did transform the imperfect metals into the perfect, taking for that operation more than a thousand years.* 2. *That all metals are composed of volatile particles* (Fumi). 3. That both parties admitted the *influence of the stars*, the alchemist only contending that they were never adverse to the making or using *the tincture*. Roger Bacon is made to affirm that each metal contains its peculiar mercury mixed with a *corruptible sulphur*, which latter may be separated by the application of the *fixed, tinged*, and *penetrating mercury*, i. e. *the tincture*. Gold itself (he proceeds) is mercury entirely freed from this sulphur, as *may be concluded from its weight, splendour, and other accidents*. The learned who have denied the existence of the philosopher's stone are briefly dismissed with the conclusive argument, that "*whatsoever they may know of other arts, they know nothing of this*."

Books XI. and XII. relate to the Hungarian and Sarmatian Schools led by Melchior Cibinensis, and an anonymous writer.†

* President of Gresham College, and a learned physician.—(See Chalmer's Biograph. Dictionary.)

† Anonymus Sarmata.

In the former, it is asserted, that there are for certain in Bohemia some nobles of Hungarian origin who derive great riches from the practice of alchemy. The mines of Chemnitz are mentioned, and a tolerably accurate account given of the various substances with which the gold of that district is naturally intermixed, and of the method of extracting it. Maier notices the care requisite in the roasting those ores which contain zinc, if united to arsenic, or other volatile matters, lest a portion should be lost in the operation. For his account of these matters, he is, I suspect, chiefly indebted to Ercker and Agricola. His philosophical rationale of the processes is sufficiently confused and incorrect. He subjoins some account of the revenues of the Turkish Empire, which he allows to result more from the art of using steel than that of making gold. The Sarmatian article is made up by an allegorical description of his travels (chiefly travels by the fireside) through the four quarters of the globe in search of his *Phœnix*. His first reason for engaging in this pursuit appears to have been the wish of ascertaining the reputed medical virtues of the *Elixir*. He ends with a prayer, some indifferent Latin poetry, and two of the hymns ascribed to Hermes Trismegistus.

The rank which Maier held in his own profession, the learning which he unquestionably possessed, and the tenour of the religious and moral sentiments which are occasionally interspersed throughout his works, forbid us, I think, to stigmatize him at once as a mere imposter, like the Cagliostros and Dousterswivels of modern history or fiction. He makes no boast of his own alchemical qualifications, nor does he (any where in this work) assert himself to have made gold, or seen it made by others. He firmly believed (I think), that the cause of alchemy was defensible both by sound argument and direct evidence; and it would be unfair to censure him too severely for not exacting, two centuries ago, the species of proof which we are accustomed to demand in matters of criticism and of natural philosophy. It is well known that hundreds both in his own day, and for the whole at least of the 17th century, participated in the same delusion. That delusion doubtless arose out of the imperfect state of chemical knowledge, and was as doubtless occasionally fostered by the arts of interested pretenders; but it is not difficult to perceive some at least of the causes which obtained for it the credence of persons destitute neither of talent nor good intentions. The powerful and singular effects of mercurial and antimonial medicines were well calculated to suggest or countenance the possible existence of a *Panacea*. The altered characters which metals assume in the state of alloys, and the obscure forms in which they exist naturally, as ores, rendered their transmutation less incredible. The cause too which contributed largely to the deception may be collected from what has been more than once noticed in the present

abstract. We have seen that all the alchemical authorities agree in the description of certain preliminary results as necessary to the completion of the *great work,* and as indisputable prognostics of success. The philosopher, therefore, believed himself to be in the way, if he had obtained the means of keeping up and moderating the heat of his furnaces, if he had effected a seeming fixation of mercury or its calces, or *extracted a tincture,* produced any thing, that is, solid or liquid, approaching to the deep-red or orange colour, supposed to be characteristic of gold. Yet more if he had procured a ponderous result of a ruby-like tinge, or even if the materials on which he operated underwent certain changes of colour in the process, did he flatter himself that he was not far from the great desideratum.

However these *signs of the work* may have been in their day sufficient inducements to so unreasonable and unprofitable a waste of time and means, your readers will have no difficulty in understanding, that they might any, or all of them, manifest themselves repeatedly in the complicated and lengthened operations of the experimentalist, without bringing him one hair's breadth nearer to the fabrication or possession of *Gold.*

I am, dear Sir, very truly yours,

J. J. Conybeare.

Article VII.

Astronomical Observations, 1823. By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Oct. 25.	Emersion of Jupiter's third satellite	12h	31′	13″	Mean Time at Bushey.
		12	32	34	Mean Time at Greenwich.
Nov. 1.	Immersion of Jupiter's third satellite	13	31	28	Mean Time at Bushey.
		13	32	49	Mean Time at Greenwich.
Nov. 12.	Immersion of Jupiter's first satellite	10	18	25	Mean Time at Bushey.
		10	19	46	Mean Time at Greenwich.

Article VIII.

On Thermomagnetic Rotation. By the Rev. J. Cumming, MA. Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy*.)

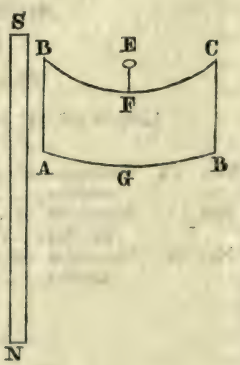
MY DEAR SIR, *Cambridge, Nov.* 18, 1823.

In the *Annals of Philosophy* for September, you did me the favour to insert a notice of two instruments for exhibiting the rotation of wires by thermoelectricity; the magnet being applied externally in the one, and internally in the other.

The parallelogram of silver and platina to which the magnet was applied externally, was attached to an agate cap, and the whole poised on the point of a long needle, in which case a counterpoise was obviously necessary. I have since found it more convenient to bend the parallelogram into the form of a semicircle, having the agate cap nearer to the wire than the centre of the circle.

A lamp and magnet being placed opposite to each other are sufficient to produce rotation; but the effect is improved by adding another magnet at 90° from the first, having its poles in the contrary direction, and being connected with it by a bar of soft iron placed beneath them. With this arrangement, the rotation will be from right to left, or from left to right, according to the position of the lamp.

A B, platina; B C F D A, silver; E, agate cap.



The second magnet is placed near F G, having its N end upwards.

If the lamp be applied beneath B, the rotation is in the direction B G A; but if it be opposite to F G, the rotation is A G B. The annexed figure represents the apparatus, which, exclusive of the agate cap, weighs about four grains.

PS. If six parts of bismuth, and one of antimony in powder, mixed together, and inclosed in a glass tube, be touched by a hot wire connected with the galvanoscope, the deviation is first positive, and then negative, as I have before mentioned to be the case with the alloy of these metals melted together in the same proportions. I am, dear Sir, very truly yours,

J. Cumming.

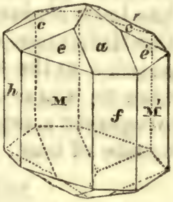
Article IX.

On the Crystalline Forms of Artificial Salts. By H. J. Brooke, Esq. FRS.

(*Continued from* p. 375.)

Sulphate of Zinc.

I am indebted to Mr. Teschemacher for some brilliant and remarkably perfect crystals of this salt, which may be cleaved parallel to the plane *h* of the annexed figure, but I have not observed distinct cleavages in any other direction.



The primary form is *a right rhombic prism.*

M on M′	91°	7′
M on *f*..............	135	33
M on *h*	134	27
M on *e*	128	58
a on *f*	120	0
h on *c*...............	119	23

Sulphate of Nickel.

I received some time since from Mr. R. Phillips some crystals of this salt, which were *right rhombic prisms;* and shortly afterwards Mr. Cooper supplied me with others which were *square prisms.* On noticing this difference of form, the first idea that suggested itself was, that there might be some difference in the proportion of water in the two salts, as both Mr. P. and Mr. C. were satisfied, from the manner of preparing them, that both must be free from impurity. The surfaces of the square prisms obtained by Mr. Cooper not being so brilliant as might be desired, he dissolved some of these crystals in distilled water, on the evaporation of which he was surprised to find it deposit *rhombic prisms similar to those I had received from Mr. Phillips,* and without the intermixture of a single square prism. On learning this fact, Mr. Phillips examined the solution from which his first crystals had been obtained, and he found that it had since deposited together others of *each of these forms,* and the crystals of *each* were observed frequently to inclose smaller ones belonging to the *other class.*

On these differences of form being discovered, Mr. Cooper and Mr. Phillips analysed several quantities of the crystals of each, and obtained nearly corresponding results, as will appear from a paper by Mr. Phillips immediately following this. Previously, however, to their analysis, Mr. Cooper reduced to minute fragments, and exposed to the air for several days, each

of the quantities he was about to examine, and he found that the rhombic prisms had lost one atom of water, while the square prisms *experienced* no loss. As the *square prisms* formed in Mr. Phillips's solution were not deposited until that had been much reduced by evaporation, it appeared probable that an excess of acid might be necessary to their production. Mr. Cooper, therefore, dissolved some of the *rhombic prisms* in dilute sulphuric acid, and from this solution *square prisms* were obtained. Thus it was ascertained that either the *square* or the *rhombic* prisms might be produced at pleasure, by crystallizing the salt from a solution in dilute sulphuric acid, or in water.

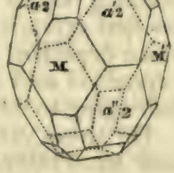
It appears from the analyses of the two sets of crystals, that between $1\frac{1}{2}$ and 2 per cent. of the water of the rhombic prisms has been replaced by sulphuric acid in the square ones. But as this difference does not constitute any *atomic* disparity of composition in the two forms, we may probably ascribe their difference to some cause analogous to that which has impressed on arragonite a crystalline form distinct from that of common carbonate of lime.

Sulphate of Nickel in Rhombic Prisms.

The form and measurements of this salt approach so very nearly to those of *sulphate of zinc*, that I am inclined to doubt of there being any real difference between them. If there be any, it will not exceed 2′ or 3′ in the inclination of M on M′, which, in many of the crystals of this salt approaches nearer to 91° 10′ than to 91° 7′. We may, therefore, refer to the measurements given above for the angles of these crystals. But there is a difference in the cleavages of the two salts, for this may be cleaved easily parallel to its *lateral primary planes*.

Sulphate of Nickel in Square Prisms.

This is the form of the crystals of this salt alluded to by Dr. Wollaston in a paper which appeared in the *Annals of Philosophy*, vol. 11, p. 286, but without any measurements. The crystals may be cleaved parallel to the planes P, M, and M′, of the accompanying figure, which are its primary planes.



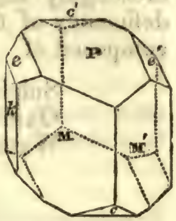
P on M, or M′	90°	0′
a_1	126	24
a_2	110	40
c	117	37
M on M′	90	0

Sulphate of Nickel and Potash.

This salt was first given to me by a friend as *sulphate of nickel,*

and I afterwards received some good crystals of it from Mr. Cooper, as a double salt.

The primary form is *an oblique rhombic prism.*



P on M, or M′	102°	15′
P on *e*, or *e*′	154	32
P on *c*′	116	17
M on M	109	10
M on *k*	125	25

Sulphate of Nickel and Zinc.

Observing the similarity of the forms of one of the sulphates of nickel and of the sulphate of zinc, Mr. Phillips dissolved equivalent proportions of the two salts in water, and obtained from the solution a new salt, having the same form and measurements as the crystals which had been dissolved. I have attempted to cleave several crystals of this double salt, but without discovering any decided cleavage planes in any direction.

Article X.

Analysis of the Sulphates of Nickel, described in the preceding Paper. By R. Phillips, FRS. L. and E. &c.

Sulphate of nickel has been several times analysed; my intention, therefore, in subjecting this salt to examination was to attempt a discovery of the causes to which the different crystalline forms it presents are referable. The composition of sulphate of nickel is stated as follows by

	Dr. Thomson.	M. Berzelius.	Mr. Brande.
Sulphuric acid	29·2	28·51	28·25
Oxide of nickel. ..	24·8	26·72	26·50
Water.	46·0	44·77	45·00
	100·0	100·00	99·75

One hundred grains of the crystals of this salt, in the form of rhombic prisms, were dissolved in water, and decomposed by nitrate of barytes; the sulphate of barytes obtained, taking the mean of two experiments, weighed 83·08 grains, equivalent to 28·16 of sulphuric acid.

Of the same salt 100 grains were decomposed by soda, and the precipitated oxide of nickel, after the requisite washing, was dried and ignited. It weighed 26·3 grains. I repeated this experiment, and obtained rather more oxide, but I had afterwards reason to suppose that the sulphate of soda formed had not been

thoroughly separated by washing. If we then consider the deficiency of the weight, as water of crystallization, the salt is composed of

Sulphuric acid	28·16
Oxide of nickel	26·30
Water	45·54
	100·00

If we suppose an atom of oxide of nickel to be = 37, the composition of the salt will be:

Sulphuric acid	28·57
Oxide of nickel	26·43
Water	45·00
	100·00

These proportions, it will be observed, do not differ much from those stated by M. Berzelius and Mr. Brande.

One hundred grains of the square prisms of sulphate of nickel were treated as above described: the mean of the experiments gave 88·65 of sulphate of barytes, equivalent to 30 of sulphuric acid; the oxide of nickel amounted to 26·2 grains. This salt, therefore, consists of

Sulphuric acid	30·0
Oxide of nickel	26·2
Water	43·8
	100·0

The excess of sulphuric acid contained in the square prisms, amounting to less than 2 per cent. cannot, I think, be considered as existing in a state of combination, but merely of mixture; and as such, we should not expect that it would influence the crystalline form of the salt.

It will be proper to state, that the result of Mr. Cooper's analysis agreed very nearly with my own, and that I confirmed the accuracy of his observations with respect to the different effects produced on these salts by exposure to the air; the rhombic prisms lost one atom of water, while 100 grains of the square prisms suffered a diminution of only one-tenth of a grain in weight.

Mr. Cooper informs me that he has analyzed the sulphate of nickel and potash, and finds that it is composed of

Sulphuric acid	37·90
Oxide of nickel	17·54
Potash	20·48
Water	24·08
	100·00

Article XI.

Substance of certain Papers on the Temperature of Mines, published in the Transactions of the Royal Geological Society of Cornwall.

When reviewing the second volume of the Transactions of the Cornish Geological Society, *Annals*, N. S. v. 295, we mentioned that a memoir would shortly appear in our journal, containing a full account of the facts detailed in four papers on the temperature of mines published in that volume. Various circumstances have interfered to prevent the completion of the memoir, and as the period of its appearance is now uncertain, we purpose, in the present article, to give the substance of three of the papers alluded to, that of the fourth, by Mr. Moyle, having already been detailed by its author in the *Annals* for January last, p. 43.

I. The first paper (Trans. GSC. ii. 14—18) is by R. W. Fox, Esq. Member of the Society.

My attention, he says, having been called to this subject in 1815, I instituted inquiries, and caused some experiments to be made in the mines of Huel Abraham, Dolcoath, Cook's Kitchen, Tincroft, and in the United Mines. The information I have thus acquired I have endeavoured to communicate in the accompanying scale, which exhibits at one view, the results which have been obtained in each of those mines.

The temperature in Cook's Kitchen and Tincroft, it may be remarked, was inferior to that in the other mines at corresponding depths; owing, I presume, to the bottom levels of the two former having been for a considerable time filled with water, accumulated, without doubt, partly from above; by which means, the temperature, not only of the water, but also of the air, in these mines, must have been affected. In the United Mines also, there was some water when the observations were made; but it remained too short a time, I apprehend, materially to affect the general temperature of the mine. Dolcoath and Huel Abraham were clear of water to the bottom; and it will be observed that the temperature in the corresponding levels of these mines differed very little, and, with a few trifling exceptions (which probably arose from local causes), the heat progressively increased, even to the greatest depths to which they have been hitherto explored.

From Mr. Fox's engraved scale, as mentioned above, the table on the following page has been drawn up, with a slight alteration in its arrangement to suit that mode of giving it:—*a* signifies the temperature of the air, *w* that of the water.

Depths from the surface in fathoms.		TEMPERATURES. Huel Abraham. June, 1815.	Huel Abraham. Dec. 1815.	Dolcoath; in argillaceous schistus and the subjacent granite: depth corresponding with level of sea about 62 fathoms. Oct. 1815.	Cook's Kitchen, in ditto; depth corresponding with level of sea, 59 fathoms. May, 1819.	Tincroft, in ditto; depth corresponding with level of sea, 59 fathoms. May, 1819.	United Mines; in argillaceous schistus containing large masses of porphyry: depth corr. with level of sea, 51 fathoms. May, 1819.
5 to	10	{58·5 / 64·5} a.	{49·5 / 51·5} a.	—	—	—	58 a.
15	25	64 a.	56·5 a.	—	—	—	—
25	30	—	—	—	49·5 a.	52·5 a	—
35	45	—	60·5 a.	—	—	—	—
40	50	—	—	—	—	—	55·5 a.
45	50	—	—	61·5 a.	—	—	—
45	55	—	62·5 a.	—	54·5 a.	—	—
50	60	—	—	—	—	51·5 a.	—
55	65	66·5 a.	63 a.	—	—	—	—
60	70	—	—	—	—	—	67·5 a.
65	75	—	—	62 a.	56·5 a.	—	—
70	75	—	—	—	—	53·5 a.	—
75	85	67·5 a.	63·5 a.	—	—	—	—
90	95	—	—	—	56·5 a.	55·5 a.	—
95	105	68 a.	{65·5 a. / 63·5 w.}	—	—	—	—
105	110	—	—	—	61·5 a.	—	—
105	115	—	{67·5 a. / 64·5 w.}	—	—	—	—
110	115	—	—	—	—	61·5 a.	—
115	125	68·5 a.	{69·5 a. / 67·5 w.}	—	—	—	—
125	130	—	—	—	62·5 a.	{61·5 a. / 58·5 w.}	—
125	135	—	71 a.	—	—	—	—
130	140	—	—	—	—	—	69·5 a.
135	145	69 a.	{71 a. / 73 w.}	—	—	—	—
145	150	—	—	—	63·5 a.	—	—
145	155	—	73·5 a.	—	—	—	—
155	160	—	—	—	—	—	67·5 a.
155	165	70 a.	{69·5 a. / 73·5 w.}	—	—	—	—
160	170	—	—	—	—	—	{72·5 a. / 73·5 w.}
165	170	—	—	—	63·5 a.	—	—
165	175	—	70·5 a.	70 a.	—	—	—
175	185	72·5 a.	{73·5 a. / 74·0 w.}	71 a.	—	—	—
185	190	—	—	—	{68·5 a. / 67·5 w.}	—	—
185	195	79·5 a.	{73·5 a. / 74·0 w.}	—	—	—	—
190	200	—	—	73·5 a.	—	—	—
195	205	—	{78·5 a. / 79·0 w.}	—	—	—	—
225	230	—	—	{79·5 a. / 81·5 w.}	—	—	—

II. The table which accompanies Mr. Fox's second paper (Trans. GSC. ii. 19—28), contains a general view of the heat observed in, or near, the metallic veins, in different mines.

I had made a second table, he remarks, containing the results of several experiments on the temperature of cross levels, and shafts, in some of the same mines, at a distance from any metallic veins; but as its contents are too various, and extensive, to be comprised in a printed sheet, I will merely mention a few of the particulars.

In Dolcoath, at the depth of 130 fathoms, where the temperature of the earth in the vein was 63°; its temperature in a cross level, at the distance of 60 fathoms from the vein, was 62°.

In the United Mines, at the depth of 160 fathoms, the temperature of the earth in the vein, was 75°; but in a cross level, south of the vein, only 69°.

In the same mines, the temperature of the water in the vein at 140 fathoms deep, was 67°; and that of the earth, 9 fathoms north of the vein, was exactly the same.

In Ting-tang, at 80 fathoms deep, the temperature of the earth in the vein was 64°; and at the depth of 110 fathoms, it was 68°; whilst in a cross level, 90 fathoms deep, and 30 fathoms distant from the vein, it was 64°.

In Huel Squire, at the depth of 110 fathoms, the temperature of the air near the vein was 72°; but in a cross level, at some distance, it was 69°.

In Treskerby, at 120 fathoms deep, the temperature of the air near the vein was 72°; but at some distance, in a cross level, 66°.

In Chacewater, the temperature of the earth in the vein at 100 fathoms deep was 82°; and that of the air at some distance north of it, 79°.

These instances, which are selected from a great number, the result of which is very similar, will suffice to show, that the temperature, at a distance from the metallic veins, and at the same depths, is, on an average, nearly three degrees below that of the veins, as given in the printed table.

In many of the observations referred to in the tables, the bulb of the thermometer was buried in the veins, or rock, to the depth of at least six or eight inches, and was filled round with earth, &c. so as to prevent the free admission of air.

If we take the mean temperature of the surface of the earth in this latitude at 53°, as given in Prof. Mayer's Tables; the mean of the accompanying table shows an increase of a little more than 6° of Fahr. for every 50 fathoms, or 300 feet in depth. As my second table gave a less ratio, perhaps we shall not much err, if we suppose an augmentation of one degree of heat for every 10 or 12 fathoms in depth, at least in this part of our island. It is however difficult to determine satisfactorily the true ratio of the increase of temperature, as it is evident that there exist many

local and accidental causes which operate in our mines, and affect their temperature. The lighted candles, and the blasting of the rocks, have doubtless some influence in augmenting the heat; and the presence of the workmen must also have the same tendency, although probably in a small degree at the bottom of deep mines, where the temperature so nearly approaches to that of the human body; moreover, the warm vapour, and air, which always arise from the bottom of mines, must raise the temperature of the upper levels in a greater or less degree, according to their relative situations. On the other hand, the currents of air which descend through some of the shafts, or are forced through the air-pipes for the supply of the miners, and likewise the water which finds its way through the strata and veins from more elevated situations, doubtless tend, in a considerable degree, to diminish the heat in the deeper levels.

How far these opposite causes may counterbalance each other, it is not easy to ascertain; but if duly considered, they will greatly reconcile the want of complete accordance in the results noted in the tables; and it is evident, that observations made on the temperature at the *bottom* of mines, are most to be confided in, not only for the foregoing reasons, but also because of the proximity of this part to the unbroken ground. There are some cases in which it cannot be supposed that the high temperature observed can be occasioned by any accidental circumstance. At the bottom of Dolcoath mine, for instance, there is a large stream of water issuing from one of the veins at 82° of Fahr. while the air near the same place is generally one or two degrees lower:—this is only one example amongst many of the same kind. The most striking one I have heard of, was reported to me by Capt. Hosken:—An accident having happened to a steam engine at the United Mines, the water increased so much as to fill the levels marked in the table 190 and 200 fathoms, under the surface; and thus it continued for two days. Immediately after it had been pumped out, and before the miners had begun to work in those levels, he ascertained the temperature of the ground in the upper one to be $87\frac{1}{2}$°, and in the lower one to be 88°. On renewing his observations some days after the men had resumed their work in these places, the heat had rather diminished than otherwise.

It is worthy of notice, that the principal part of the work is not always carried on in the deepest part of the mines: on the contrary, there are often more workmen employed at twenty or thirty fathoms above the lowest part, than in the deepest level. If therefore the increase in the temperature were wholly the effect of adventitious causes, that increase would be greatest where those causes had their largest operation; but the facts which I have detailed in the table, prove that, however various may be the operation of accidental circumstances in different parts of the mines, the temperature invariably increases with the depth.

My friend and relative, Joseph T. Price, of Neath, Glamorganshire, has furnished me with the results of some observations made last spring, in three collieries in the neighbourhood of that place. The thermometer was buried for many hours from one to two feet under the ground, at the bottom of each of these collieries: in one of them, which was only 10 fathoms deep, the mercury stood at 50°; in another, 36 fathoms deep, it stood at 58°; and in the third, which was 90 fathoms deep, it stood at 62°. The difference between the first and last mentioned collieries was 12°, which ratio nearly corresponds with that obtained in our mines.

It has been surmised that the heat noticed in the mines may be attributed to the presence of metallic, and other inflammable substances; but when all the facts are considered, no causes, merely local, can be imagined capable of producing such constant, extensive, and powerful effects. If the water received its heat from the metallic veins, while passing through them, it would surely become strongly impregnated with mineral substances;—this is not, however, the case. I analyzed some from the deepest part of Dolcoath, taken at 82°, immediately from the copper vein, and obtained from a quarter of a pint of it only half a grain of residuum, consisting of sulphuric acid, some oxide of iron, and a little lime. I found a greater proportion of the same substances in some water from a cross level, at a distance from any vein, 200 fathoms deep, in the same mine.

Water from the bottom of the United Mines, of the temperature of 82°, contained six grains of muriate of lime in a quarter of a pint. Some water taken from the deepest parts of Treskerby and Ting-tang, was, from the former mine, very slightly impregnated with sulphate of iron, and had a trace of muriatic acid; and that from the latter mine contained a very minute portion of the muriate of lime.

Since my last communication on the subject of the temperature of mines, I have had a thermometer, four feet long, placed in a hole three feet deep, in a copper vein, at the end of the deepest level, or gallery, in Dolcoath, which is 230 fathoms, or 1380 feet, under the surface; a spot where no workmen were employed, and where the current of air must have been small. The hole was filled with clay round the stem of the thermometer, so as to prevent the circulation of air near the bulb, and in this situation it remained more than eight months. It was often examined during that period, and was always found to indicate a temperature of 75°, or 75½°, unless it had been recently overflowed by water. This happened several times, in consequence of accidents to the machinery of the mine, and more than once, the water filled the level for some weeks. As soon as it had subsided, so as to permit access to the thermometer, the quicksilver was observed to have risen to 77°, but in two or three days it again fell to 75½°.

Table of the Temperatures of sundry Mines at different Depths as taken in or near the Veins.

Depth in fathoms.	Dolcoath. Copper vein. Dec. 1819.	United Mines. Copper vein. April, 1819.	Treskerby. Copper vein. Dec. 1819.	Huel Squire. Copper vein. Sept. 1820.	Ting-tang. Copper vein. Sept. 1820.	Huel Gorland. Copper vein. Sept. 1820.	Huel Damsel. Copper vein. Sept. 1820.	Chasewater. Tin & Copper. Sept. 1820.	Huel Unity. Tin vein. Aug. 1820.	Huel Vor. Tin vein. Nov. 1819.
1 to 10										S. w. 52°
10 20										
20 30										S. w. 61
30 40		S. e. 56°			S. a. 62°			S. a. 61°		
40 50	S. e. 58°									
50 60		S. a. 63					G. a. 61°			
60 70		S. a. 64					G. a. 61		S. a. 66°	
70 80				S. a. 67°	S. e. 64	G. e. 62°	G. a. 66	G. a. 70		
80 90		S. a. 65					G. a. 69	S. a. 78	S. e. 66	S. w. 63
90 100	S. c.* 58		G. e. 67°	S. e. 72			G. a. 70	P. e. 82		
100 110			G. 70	S. a. 72	S. e. 68	G. a. 68	G. a. 70			S. w. 64
110 120			G. a. 72	S. {a. 72 / w. 68}			G. a. 70			S. w. 66
120 130	S. e. 63		G. a. 76				G. a. 70			S. w. 70
130 140		S. w. 67	G. w. 6				G. a. 73			S. {w. 69 / a. 72}
140 150							G. a. 70			
150 160		S. e. 75								
160 170										
170 180		S. w. 7								
180 190		S. e. 86								
190 200	G. e.* 64	S. e. 87								
200 210										
210 220										
220 230	G. e. 78									
230 240	G. w. 82									

NB. The capital letters denote the rocks which inclose the veins, viz. G. Granite, S. Schist, P. Porphyry. The small letters a. e. w. show that the temperature is that of the air, earth, or water.

* Here there was a strong current of air.

III. The third and last paper we have to abbreviate is by Dr. J. Forbes, late Secretary of the Society (Trans. ii. 159—217).

1. *Huel Neptune.*—Copper mine, situated in killas. Height above the level of the sea, about 200 feet. Depth, at the period the observations were made, 550 feet. (Depth in 1822, 750 feet.) Number of men employed under ground 120. Expenditure of candles per month 1200 lbs. Expenditure of gunpowder ditto 250 lbs. Quantity of water discharged per day 216,000 gallons. Temperature of this at the adit at the time the observations were made 60°. (in 1822, 62°.) Has been working 11 years (1822).

2. *Botallack.*—Tin and copper mine. Height above the level of the sea about 40 feet. Depth at the time the observations were made, 570 feet. (Depth in May, 1822, 672 feet). Number of men employed underground 150. Expenditure of candles per month 1200 lbs. Expenditure of gunpowder ditto 600 lbs. Quantity of water discharged by the pump daily 57,600 gallons. Temperature of this at the adit in 1819, 62°, in 1822, 67°. Has been worked 17 years (1822).

3. *Little Bounds.*—Tin mine. In killas and granite. Height above the level of the sea 72 feet. Adit at the sea-level. Depth from the surface 504 feet. Number of men employed under ground 25. Expenditure of candles per month 48 lbs. Expenditure of gunpowder ditto 60 lbs. Quantity of water discharged by the pumps daily 69,000 gallons. Temperature of this water $55\frac{1}{2}$°.

This mine has been worked 30 years, but very little has been done in it of late years, and the water has consequently risen in it, to the 40 fathom level under the adit, that is, to the height of 192 feet from the bottom : it is kept under at this level by the partial action of the engine.

4. *Ding Dong.*—Tin mine. In granite. Height above the sea level about 400 feet. Depth from the surface 606 feet. Number of men employed underground 120. Expenditure of candles per month 900 lbs. Expenditure of gunpowder ditto 300 lbs. Quantity of water discharged by the pumps daily 50,000 gallons. Temperature of this at the adit, 61°. Has been worked eight years.

5. *Huel Vor.*—Tin mine. In killas. Depth from the surface 948 feet. Number of men employed underground 548. Expenditure of candles per month 3000 lbs. Expenditure of gunpowder ditto 3500 lbs. Quantity of water discharged per day 1,692,660 gallons. Temperature of this at the adit 67°. Has been worked 12 years.

6. *Dolcoath.*—Copper mine. In killas. Height above the level of the sea about 300 feet. Depth from the surface in 1819, 1386 feet; in 1822, 1428 feet. Number of men employed underground (1822) 800. Expenditure of candles per month 6000 lbs. Expenditure of gunpowder ditto 2600 lbs. Quantity of water discharged by the pumps daily 535,173 gallons. Temperature

of pump water at the adit (1822), in the eastern part of the mine, 72°; in the western ditto (much shallower) 64°. Has been worked 20 years.

Mean Results of the Temperature of Six Mines.

Depth in feet.	Huel Neptune.		Botallack.		Little Bounds.		Ding Dong.		Huel Vor.		Dolcoath.		Mean.	
	a.	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*
120 to 150	57		59	57			55						57	57
150 200	56		60	58	54	54							57	56
200 250	56		61		57	55			60	57			58	56
250 300	56	55	61	59	57	59					60		58	58
300 350	58	54			55	55							57	55
350 400		57	66	62			55						60	59
400 450	60		66				56	54					61	54
450 500							60	59					60	59
500 550	67	67	67	68					61	60	64		65	65
550 600							63	63					63	63
600 650							62	63	61	63			62	63
650 700							64	64	65	64			65	64
700 750									67	65			67	65
750 800									68	68			68	68
800 850									66	66			66	66
850 900										68			*	68
900 950										71	62		62	71
950 1150											70	66	70	66
1150 1260											71	71	71	71
1260 1350											76	74	76	74
1350 1400											83	79	83	79

B.

Article XII.

On the Occurrence of Cleavelandite in certain British Rocks. By W. Phillips, FLS. MGS. &c.

Some specimens of the rock of Mount Sorrel, in Leicestershire, lately brought from that place by my friend S. L. Kent, MGS. and myself, appear to be chiefly constituted of two varieties of felspar, common, and glassy. A third variety in other specimens is reddish or red, sometimes extremely red, and nearly opaque. These appeared to us to be only varieties of the same mineral, and believing that mineral to be felspar, we should so have designated the whole in a communication which we purpose shortly to send for insertion in the *Annals,* on the rocks in question, and on those of Charnwood Forest, but for the remarks of M. Levy in the number for November, showing that much which had been considered as felspar is in fact cleavelandite.

* Here there was a strong current of air.

Felspar and cleavelandite, it is remarkable, agree in their earthy ingredients, which exist very nearly in the same proportions in both substances; but they differ in this, that the 13 or 14 per cent. of potash in felspar is substituted by about 10 per cent. of *soda* in the cleavelandite, which, moreover, is not so hard as felspar. These minerals possess natural joints parallel to the planes of the doubly oblique prisms, which are considered to be the primary forms of the two minerals; but these forms differ so completely in the measurements of all their angles, that there is no hazard of mistaking the one for the other, after submitting them to the reflective goniometer. This, in consequence of M. Levy's paper, we have done, separately, and with care, and we find that in the Mount Sorrel rock, felspar and cleavelandite are intermixed; but it is impossible for us even to guess their proportions as ingredients, since for the most part it is difficult, frequently impossible, to separate them by the eye. It may, however, be observed, that the felspar is frequently translucent or transparent, and often reddish; the cleavelandite, white or yellowish-white, and nearly opaque, or various shades of red, and that the very red veins traversing the rock here and there, are chiefly of this mineral. Abundance of coinciding measurements on fragments of both substances satisfy us of their aggregation in this rock.

I have since sought for the cleavelandite in other rocks, and have found it, as well as felspar, in a beautiful porphyry from Glen Tilt; the specimen was obligingly presented to me some years ago by Dr. Mac Culloch. In this specimen it is both transparent and colourless, and red and opaque. I have also detected it in a porphyritic granite from Carnbrae in Cornwall: in this specimen it is translucent and colourless, and white and opaque, and felspar is more abundant in it. In the granite of Shap, in Westmoreland, there is an intermixture of a whitish or yellowish-white substance, of which some very minute and dull fragments have afforded measurements within one degree of those of the cleavelandite; and I do not hesitate to believe that better specimens would prove it to be that mineral.

Article XIII.

On some Thermomagnetic Experiments. By Dr. T. S. Traill.

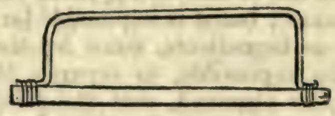
(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Liverpool Royal Institution, Nov.* 21, 1823.

Having been lately engaged in some thermomagnetic experiments, I have met with results which none of the papers

on the subject that I have perused led me to expect. Should they appear to you sufficiently important, I transmit a short account of them for insertion in the *Annals*.

The apparatus which I have found most convenient consists of a bar of antimony $4\frac{1}{2}$ inches long, half an inch broad, and one-quarter of an inch thick. To this, a slip of copper equally broad, and bent as in the figure, is firmly attached by a few turns of copper wire. This method of connecting them is better than by solder; because the joinings can then bear a higher temperature. A spirit lamp is the source of heat, and the deviations are observed with a delicate pocket compass, the needle of which is $1\frac{1}{2}$ inch long, and poised on an agate centre.



When the apparatus is placed in the magnetic meridian, with the slip of copper uppermost, as in the figure, and the lamp is applied to the N end of the bar, the needle placed *within* the rectangle, always deviates to the W; while the compass placed in contact with any part of the *outside* of the rectangle (whether copper or antimony) deviates to the E.

These effects are reversed when the lamp is applied to the south end of the apparatus, other circumstances remaining the same. While the metallic surfaces in contact are bright, the deviation often amouuts to 75° *within* the rectangle, while *without* it, the deviation is usually 45°, or upwards; and the effect produced by the upright portions of the copper connecting piece, is less than of the horizontal parts of the same metallic slip.

Absolute contact of the compass with the metallic apparatus is not necessary. The effects were apparently as powerful when the compass was placed on a plate of glass about half an inch in thickness; or even when held in the hand, without touching any part of the apparatus, especially when held within the parallelogram. Hence the magnetic power of such a combination extends to some distance from its surface, like the magnetism of a common magnet.

Inclining the apparatus at different angles from 20° to 72°, produced no change in the deviations, provided the direction of the apparatus was in the plane of the magnetic meridian.

The inversion of the apparatus so as to place the antimony *above* the copper was then examined. When the N end of the bar was heated, and the compass on the upper, now outer surface of the antimony, and on all the outer surfaces of the rectangle, the deviation was still to the W, and all the interior surfaces of the rectangle showed deviation to the E. When the

south end was heated, the effects were reversed. When the apparatus was laid horizontally with the antimony on the magnetic meridian, and the lamp applied to the N end, the compass placed on the upper surface of the antimony deviated to the W; and when placed on the copper of the opposite limb to the E; whether the copper was to the W or the E of the antimony. Heating the S end reversed the effects.

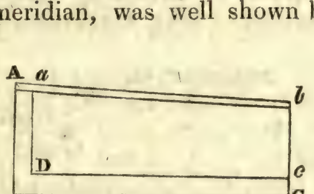
From the care with which my friend Prof. Oersted appears to have placed one side of his compound apparatus in the magnetic meridian, and the notice of this arrangement by other philosophers, I was led to believe that it was essential to the success of these experiments that one of the bars should be in that line; and my first experiments, with a smaller apparatus, induced me to believe that there is no deviation of the needle when the apparatus is placed at right angles to the meridian; but on repolishing the surfaces of the metals, where in contact, and applying the spirit lamp for a longer time, I found that idea to be erroneous. *The apparatus acts most powerfully when placed at right angles to the magnetic meridian.*

When so placed, and the copper connecting wire uppermost, I applied the lamp to the W end of the apparatus; and though the needle within the rectangle appeared quite stationary, for considerably longer than in the former experiments, it soon began to deviate, and at length had its poles inverted; making short oscillations, showing a strong magnetic intensity.

When the lamp was applied to the E end of the apparatus, the needle within the rectangle did not move; but when disturbed, it made short oscillations, indicating that it was acted on by magnetism independently of the influence of the earth.

On inverting the apparatus, so as to have the antimony uppermost, and continuing the heat to the E end, the needle placed in the rectangle had its poles speedily inverted; and it was afterwards found, that when the apparatus remained in this inverted position, and the lamp was applied to the W end, no deviation was produced, though the needle vibrated more quickly than usual.

That the apparatus was most powerful, when at right angles to the magnetic meridian, was well shown by another form of the apparatus.



A B C is a bar of antimony, with a right angle at B. To its extremities was soldered a slip of copper, *a b c*, as in the figure, forming a parallelogram. When A B was in the magnetic meri-

dian, and the lamp applied at A, the compass placed on A B deviated to the W about 55°; when placed over the elbow B D it deviated 90°; and when placed in the middle of B C, it was inverted. I am, dear Sir, yours truly,

Thomas Stewart Traill.

Article XIV.

Analyses of Books.

Meteorological Essays and Observations. By J. F. Daniell, FRS.

In declaring meteorology to be yet in its infancy, nothing is less intended than to convey a reproach against the cultivators of that promising and interesting field of science; for it must be remembered, that the instruments by which it is explored, are either altogether of recent invention, or have only of late been rendered, by improved construction, and the establishment of formulæ for correcting their still unavoidable errors, susceptible of that degree of precision which is essential to accurate results. It is a field, indeed, from which there cannot be a doubt that, at some remote period, a rich harvest will accrue of knowledge most important to the interests of mankind, and exalting still higher the dignity of man, as the only being, to whom it is permitted to understand the laws by which the universe is governed, and, out of seeming confusion, to educe a system of magnificent extent and of perfect order. Every one, therefore, is entitled to share in the glory of this great achievement who either gives a distinct view of what is already known, and points out what remains to be explored; or who increases the delicacy and correctness of the instruments of investigation; or, by patient and multiplied observations, supplies data for general principles. But to establish such principles, there will be required a most extensive co-operation among observers in almost every part of the habitable globe, and an unceasing watchfulness over atmospherical phenomena for a long succession of years, if not of ages.

Among the most important objects to which the constant attention of the meteorologist requires to be directed are the fluctuations of our atmosphere as to weight, temperature, and moisture, at any particular spot, and at various elevations; the changes that take place in the distribution of its general mass giving rise to winds both regular and inconstant; the precipitation of its aqueous contents by the commixture of masses of air of different temperatures; and the influence of various causes upon spontaneous evaporation. From the vapours that float on the surface of the earth, he raises his view to higher regions,

observes and classifies the forms of clouds, remarks their motion, their production and disappearance; and from these indications he is often enabled to foretel changes of weather, which to the careless observer seem the wayward results of chance and accident. Besides these more constant phenomena, it is his business to examine and record the occasional ones of thunder and lightning, the aurora borealis, and other luminous meteors, which, though seemingly casual, are no doubt parts of a regular chain of events, which we may hope to see one day spread before us in unbroken continuity. In the study of atmospherical phenomena there is, therefore, a wide scope for the ingenious inventor of refined and delicate instruments; for the careful and patient observer of facts; and for him also who is capable of taking a wider range, and of connecting individual truths into an harmonious and durable system.

In our own language there are but few works that have been exclusively devoted to the subject of atmospherical phenomena. In 1787, Mr. Kirwan published a small octavo volume entitled, "An Estimate of the Temperature of different Latitudes," which, among some errors, contains much valuable matter, collected with great pains from a variety of sources. In 1793 appeared a small volume by Mr. Dalton, entitled, "Meteorological Observations and Essays," which does no discredit to the subsequent fame of that distinguished philosopher. This was followed, after an interval of several years, by Mr. Forster's "Researches," and Mr. Luke Howard's valuable work, "On the Climate of London." To these, indeed, may be added several detached treatises in the different Encyclopædiæ, under the heads of Barometer, Climate, Cold, Hygrometry, Meteorology, Rain, &c. and in the Transactions of the Royal and other Societies, and the various periodical journals, a great mass of useful information is spread over a wide surface. It would be a most acceptable service, therefore, to the meteorological inquirer, if all this scattered knowledge were reviewed and methodized. He would then be placed on an eminence, from which, surveying what is known, he would be able to mark the bearings of unexplored regions. This kind of history is not the object of Mr. Daniell's work, which is rather to be considered as a train of original investigations; and antecedent discoveries are related, chiefly as they bear upon the subjects of his inquiries. In these inquiries he has shown considerable ingenuity and great industry; and if we should doubt of the soundness of some of his conclusions, or the value of a part of his labours, it is still with feelings of respect for the general merits of his performance, and with approbation of the fairness and candour with which he has treated those who have written before him on the same topics.

The work consists of five separate essays: 1. "On the Constitution of the Atmosphere." 2. "On the Construction and Uses of a new Hygrometer." 3. "On the Radiation of Heat in

the Atmosphere." 4. "On the Horary Oscillations of the Barometer." 5. "On the Climate of London." To these are added a collection of meteorological observations in tropical climates by Capt. Sabine and Mr. Caldcleugh; some remarks on the barometer and thermometer; observations upon heights; and a meteorological journal of three years kept by Mr. Daniell. The first essay, which treats of the constitution of the atmosphere, is divided into four parts. Under the first are considered the habitudes of an atmosphere, of a perfectly dry and permanently elastic fluid; in the second those of an atmosphere of pure aqueous vapour; in the third, the compound relations of a mixture of the two; and in the fourth, the principles which have been derived from these inquiries are applied to the phenomena of the mixed atmosphere of the earth.

After recapitulating those statical laws of elastic fluids which were first developed by Newton, Mr. Daniell proceeds to calculate the influence of temperature in modifying the density and elasticity of air at different elevations. The principle from which the necessary data are derived was pointed out originally by Mr. Dalton in his "New System of Chemical Philosophy," Part I. p. 123. It is there conjectured, that "the natural equilibrium of heat in an atmosphere is when each atom of air, in the same perpendicular column, is possessed of the same quantity of heat; and consequently, owing to the increased capacity produced by rarefaction, the natural equilibrium of heat is when the temperature gradually diminishes in ascending." The formula, however, on which the calculations of Mr. Daniell are founded, was furnished by Prof. Leslie. But it may be reasonably doubted, whether the experimental process of which this formula expresses analytically the result, be susceptible of the necessary accuracy.

Hitherto the temperature of the sphere round which this imaginary atmosphere is diffused, has been supposed to be uniformly the same on every part of its surface. The hypothesis is now, however, to be changed; and we are to contemplate a sphere the temperature of which being 0° Fahr. at the poles, increases *by equal degrees* till it becomes 80° at the equator. From this supposition, the conclusion immediately follows, that the atmospheric column over the polar regions will be shorter and denser than that over the equator; and consequently that an inferior current of cold air will flow uniformly from the poles to the equator. At a certain elevation, the greater *density* of the polar air will be exactly counterbalanced by the greater *elasticity* of the equatorial; and of this equilibrium of forces, perfect rest must necessarily be the result. Above this quiescent point, a current in the opposite direction, viz. from the equator to the poles, will manifestly be established. This constant and regular flow, according to Mr. Daniell, modifies in no respect the height of the mercurial column.

We are next to imagine a sphere increasing in heat *unequally* from the poles to the equator. In this case, the currents will set as before, and at nearly the same altitude, but with unequal velocities in different parts of their course. The height of the barometric column at the surface will still be invariable; if as has been so far supposed, the heat be communicated to the atmosphere immediately from the sphere, and be slowly transmitted from the lower to the upper strata. But the influence of any partial and temporary source of heat, the agency of which is entirely confined to the higher regions of the atmosphere, will produce a different train of phenomena. This local increase of heat will augment disproportionately the elasticity of the superior strata, and will, therefore, disturb the regular flow of the equatorial current. A fall of the barometer wherein this disturbance takes place will be a necessary consequence of the diminished density of the atmospheric column.

The second part of Mr. Daniell's first essay is devoted to the consideration of an atmosphere of pure unmixed aqueous vapour. If the temperature of the sphere be supposed to be 32° on every part of its surface, the experiments of Mr. Dalton have shown that the elastic force of a vaporous atmosphere would at the surface be equal to 0·2 of an inch of mercury. The density of such an atmosphere would, from statical principles, decrease in a geometrical progression for equal heights. But supposing the temperature of the sphere to increase as before from the poles to the equator, it is evident that on the principle of the cryophorus, the elasticity of the whole vaporous atmosphere would be determined by that at the lowest point. Mr. Daniell supposes, therefore, that the passage of the vapour from one point to another is mechanically retarded, so as to enable it to assume the gradations due to the temperature of the subjacent part of the sphere. The direction of the currents would, in this case, be the reverse of that of a permanently elastic fluid, and they would flow from the equator to the poles, instead of from the poles to the equator. For increase of temperature augments both the density and elasticity of aqueous vapour, when in contact with water; whereas in a free atmosphere of a permanently elastic fluid, increased elasticity is always accompanied by diminished density. At different elevations, the aqueous vapour would naturally assume the temperature due to its density. But if the heat of the higher strata be supposed to be diminished by any cause at a greater rate than is due to this natural gradation, a partial condensation must necessarily ensue.

Under the third division of Essay I. Mr. Daniell proceeds to inquire into the relations of a compound atmosphere, formed by the combination of aqueous vapour with a permanently elastic fluid. The basis of this investigation, as of the two former, is founded on the discoveries of Mr. Dalton. That philosopher (in his "New System," p. 150) was the first to reject the com-

mon hypothesis of the chemical union of mixed gases, and to substitute in its room a theory better according with observed phenomena, and established by a series of new and important experiments. The leading principle of this theory is, "that the particles of one gas are not elastic or repulsive in regard to those of another gas, but only to the particles of its own kind." Hence it is inferred, that the gases which constitute our atmosphere exercise no further action upon each other than a mechanical opposition when in motion. The aqueous vapour will then be subjected to no additional pressure by commixture with a permanently elastic fluid. It will, however, be greatly modified by the temperature of the gaseous atmosphere. For example, at an elevation of 5000 feet, the density of an unmixed atmosphere (that at the surface being taken as unity), would be 0·897 of an inch, and its temperature consequently 76·5° Fahr. The temperature of an atmosphere of a permanently elastic fluid would however, at the same elevation, be only 64·4°. A mixture of the two atmospheres must then be necessarily accompanied by a condensation; for vapour of ·897 density could not subsist at a temperature of 64·4°. Supposing this condensation to have taken place, and each stratum of air to possess the exact quantity of moisture due to its temperature, the two atmospheres will still be in a state of intestine motion. For the elasticity of the vapour formed at the surface of the sphere not being counterbalanced by an equivalent pressure from above, that vapour must be continually ascending into the higher regions of the atmosphere, where it will be condensed, and will give out its heat to the ambient air. A reference to our former example may serve to elucidate this general position. It appears from the calculations of Mr. Daniell, that the natural state of an atmosphere of pure aqueous vapour diffused around a sphere of the uniform temperature of 80°, would require, at the elevation of 5000 feet, vapour of the density ·897. Under these circumstances, the pressure of the superior strata would exactly balance the upward tendency of the lower, and perfect rest would necessarily result. But in a mixed atmosphere, it has been already shown, that the density of vapour, at an equal elevation, would be only ·636, or what is due to the temperature of 64°. Hence the pressure of this vapour will not be adequate to counteract the expansive force of the lower strata. Therefore the vapour formed at the surface will ascend into the colder regions, will be there condensed, and will impart its constituent heat to the surrounding medium. Here then is to be found the partial source of heat, to which a tacit reference has been made in the first part of the essay. The elasticity of the higher strata of the atmosphere will be augmented by this accession of temperature, and the velocity of the equatorial current will receive a disproportionate increase. To the irregularities of pressure thus produced are attributed by Mr. Daniell the fluctuations of the barometer.

We have thus endeavoured to give a concise view of a theory framed to account for the changes which are constantly taking place in the pressure of the atmosphere. It has certainly the merit of ingenuity, and, so far as we are aware, of novelty, but it rests upon the sandy foundation of assumed partial changes of temperature in the higher regions of the atmosphere, of the existence of which we have very insufficient evidence, and which, moreover, if they were by any train of reasoning rendered probable, could scarcely be considered as adequate to explain the phenomena. For to evolve so much heat as would raise the temperature of a considerable mass of air, and cause it to diffuse itself rapidly into distant regions, would require the condensation of a greater quantity of aqueous vapour than is likely to be present in any given space, and also that this condensation should not be gradual, but should take place suddenly to a very great amount. There can be no discredit, however, to any one who fails to unfold the causes of phenomena which have been acknowledged by one of the first philosophers of the present times * to have hitherto baffled all attempts to reduce them to fixed principles. The data for a sound and stable theory are, it appears to us, still wanting, and must be supplied chiefly by a very extensive series of simultaneous observations on the state of the barometer, in various and distant parts of the world.

We may remark, by the way, an error, as it seems to us, into which not only Mr. Daniell (p. 8), but Mr. Leslie, has fallen, viz. "that the particles of air in passing over the surface of the globe do not for a moment cease to gravitate, and that no horizontal movement of them will produce the slightest derangement in a perpendicular direction." Now it is well known that any body, to which a projectile motion of five miles per second has been imparted, would revolve around the earth like a planet, and would cease to exert any pressure on its surface. Any less velocity must produce a proportional decrease of weight in the particles of air, which is known to move at the rate of from 60 to 100 miles per hour.

We venture also to suggest, with submission, that the third table in Part I. is founded on an erroneous principle. In calculating the influence of a decreasing temperature on the weight of the atmosphere at different heights, Mr. Daniell has deducted $\frac{1}{480}$ of the length of the mercurial column for each degree of depression due to the elevation. Now it appears to us, that a mean ought to have been taken between the temperature at the base, and that at the summit of the atmospheric column. For example, the weight of a column of air of 5000 feet, supposed of an uniform temperature of 32°, and decreasing in density from the surface upwards, according to statical laws, is equal to

* M. Biot.

5·203 inches of mercury (see Table I, p. 13, of Mr. Daniell's work). The barometer, therefore, which, at the surface of the supposed sphere, stands at 30 inches, will, at this elevation, indicate 24·797. But if the temperature of this aerial column gradually decrease from 32°, till, at the height of 5000 feet, it becomes 14·8°, it is required to determine the change which this variation will produce in the height of the mercurial column at the above elevation. The question seems to us to reduce itself to a simple comparison between the weight of a column of air 5000 feet high, of the temperature 32° Fahr. and that of an equal column of the temperature 23·4°, which is the mean of the temperature at the base and that at the summit. Now air by being reduced 1° Fahr. contracts in bulk $\frac{1}{480}$ of the volume which it would occupy at 32°; consequently a reduction of temperature equal to 8·6 ($32'' - 23''\cdot 4$) will be accompanied by a decrease of volume equivalent to $\frac{8\cdot 6}{480}$ of its former bulk. The vacuous space which would be left by such a contraction must be immediately filled up by air from above. Hence the mercurial column at 5000 feet must, by falling, indicate this transference of air from the superior to the lower strata, and this fall will be equal to $\frac{8\cdot 6}{480}$ of $5\cdot 203 = \cdot 093$. At the elevation of 5000 feet then, the height of the barometrical column will be equal to $30 - \overline{5\cdot 203 + \cdot 093} = 24\cdot 704$, instead of 23·949, the number given by Mr. Daniell. The same result will be obtained by means of a formula derived algebraically from one originally given by Sir G. Shuckburgh.* Let H denote the height of the mercurial column at the surface of the earth, y that at a given elevation p (in the present instance 5000 feet), and b the number of feet of air of the given temperature ($23''\cdot 4$), equal to 1-10th inch mercury.

Then $y = \frac{600\,b - p}{600\,b + p} \times \text{H}$. Substituting in this formula the values of b and p, the former of which is obtained from a table given by Sir G. Shuckburgh, we have $y = \frac{600 \times 85\cdot 044 - 5000}{600 \times 85\cdot 044 + 5000} \times 30 = 24\cdot 64$. The small difference between this result and the former one may be attributed to Sir G. Shuckburgh's having estimated the expansion of air for each degree Fahr. at $\frac{1}{435}$ instead of $\frac{1}{480}$ of its original bulk.

Our limits will not permit us to enter at any length into the account of Mr. Daniell's hygrometer, which is fully described in his work, and also in the Quarterly Journal, Nos. 11 and 25 We consider it as an elegant instrument, and are satisfied by

* Dalton's Meteorological Essays, p. 82.

trial of it that it is adequate to its object, that of ascertaining quickly and correctly the temperature at which dew begins to be deposited. But we are not aware that in accomplishing this, it has any great advantage over the method of Le Roi, which is recommended by the extreme simplicity of the apparatus required. This consists of nothing more than a thermometer and a glass tumbler filled with water, the temperature of which is lowered by gradually adding ice (nitre or sal-ammoniac would answer the same end) till dew begins to appear on the outer surface of the vessel. Noting this point, whether obtained by Le Roi's or Mr. Daniell's method, we then find, from Mr. Dalton's table, the force of vapour at that temperature; and from the proportion which this force forms of the whole pressure of the atmosphere at the time, we at once arrive at the absolute quantity of vapour in a given space. We regard the indications of this simple process as much more satisfactory than those of Mr. Leslie's hygrometer, because, to deduce from the latter the real proportion of vapour in air, requires a much more complex calculation, of which some of the data, or of the steps, may possibly be erroneous.

The remaining essays of Mr. Daniell we are obliged to pass over without any notice. Indeed being chiefly composed of details of facts, they are not from their nature susceptible of abridgment. They are important, however, to those who are practically engaged in making or recording meteorological observations, and to all such persons, as well as to those who are interested in the theory of atmospheric phenomena, we can safely recommend the work as containing an ample fund of valuable information. Z.

The Elements of Pharmacy, and the Chemical History of the Materia Medica, &c. By Samuel Frederick Gray, Lecturer on the Materia Medica, Botany, and Pharmaceutic Chemistry.

It is impossible to deny that this work is calculated to convey a considerable portion of information; but it must at the same time be admitted, that much of it will be of little use to the student. The arrangement (if indeed arrangement it can be called) is peculiar, and while some subjects are treated of with extreme brevity, there are others which are extended much beyond the requisite limits; thus weights, measures, and balances, occupy about 20 pages, furnaces 33, and the theory of chemistry 34. The properties of atmospheric air and water are then detailed; lead, copper, tin, and some other metals, are next treated of in six pages; and we are then surprised with an account of the "alchemy of the Greek clergy," "the introduction of alchemy into the west," and the "original theory of transmutation;" these disquisitions occupy about seven pages.

It is a bad omen to stumble at the threshold, but we cannot help it: Mr. Gray thus defines chemistry:—"The alterations and appearances that take place in the admixture of bodies, and the action of heat and cold upon them, are the proper objects of chemistry; which also endeavours to explain the production of similar phenomena when they arise from other causes."

Now unless cold be a positive power, which we suppose Mr. Gray will not contend that it is, the effects of cold are referrible to alterations of temperature, and consequently to the subject of heat itself; what the similar phenomena are which arise from other causes besides chemical action, we are quite at a loss to conjecture.

After giving the theory of combustion, which we must pass over without remark, Mr. Gray proceeds (p. 189) to the consideration of the

"*Compound Combustibles.*—The more simple substances being thus gone through, it remains only to treat of those compound combustibles, which are, generally speaking, produced in organic bodies, or from bodies having that origin. Some of them, indeed, are so loaded with water or other incombustible matter, as vinegar or oyster shells, that they appear, to a common observer, to be themselves incombustible; but when the water or other extraneous matter is separated, this appearance vanishes. In point of chemical composition, they are, generally speaking, compounds of carbon, hydrogen, and oxygen, to which are sometimes added nitrogen and other ingredients: hence they are distinguished from the combustibles of the former series, in always forming both carbonic acid and water by their union with more oxygen."

Some further observations succeed the above, and we then arrive at the "Pharmaceutical Division of Combustibles;" and Mr. G. informs us, that the divisions which the "pure chemists" have formed, are not followed in his work. "Spirit of wine and vinegar, being of continual use in chemistry, as agents in the preparation and examination of bodies, are first noticed; and the remainder of the combustible bodies are arranged according to their taste, as being the quality that is usually first attended to in examining them, and which has also a considerable connexion with their medical virtues. For the sake of elementary brevity, scarcely any other of these articles but those enumerated in the Materia Medica of the London College of Physicians are noticed. The arrangement of these combustible drugs is as follows:

1. Earthy and absorbent bodies.
2. Farinaceous, mucilaginous, gelatinous, gummy, and emollient bodies.
3. Bitter bodies.
4. Austere and acerb bodies.
5. Acid bodies.
6. Aromatic bodies.

7. Fat and oily bodies.
8. Sweet bodies.
9. Acrid bodies.

It will, perhaps, be scarcely credited when we state, that the first "combustible drug," which its "taste" has assigned a place among the earthy and absorbent bodies, is incombustible and tasteless. When treating of "the earthy and absorbent bodies," Mr. Gray says, "only one compound combustible substance of this kind is now quoted in the London Pharmacopœia, namely, testæ.

"*Testæ.*—Oyster shells consist of carbonate of lime deposited in a tissue of gelatinous matter, which latter is very small in quantity; hence they are used only as antacids. On calcination, the gelatinous matter is burned, the carbonic is driven off, and a pure lime remains." Now as chalk is quoted in the London Pharmacopœia, as well as oyster shells, the reader will wonder with us how it happened not to be arranged with the earthy and absorbent bodies; it is true that it has no taste, and is not combustible; but it is at least as sapid and as combustible as oyster shells; the reason we suppose to be, that as shell contains a small quantity of gelatinous matter which is combustible, but to which it owes none of its absorbent powers, it is ranked among the compound combustibles. We think we need scarcely ask, whether any arrangement can be essentially good which separates two varieties of carbonate of lime, because one contains an admixture of gelatinous matter.

The substances brought together under the name of farinaceous bodies, are as dissimilar as bodies can be. Among them are gum arabic, wax, horns, henbane leaves, and eggs.

Arrangement, however, is a matter of secondary importance, provided the substances when met with are accurately described; but there are many instances of inaccuracy in Mr. Gray's work, some of which, taken at random, we shall point out, premising, however, that we did not expect to find phosphorus and sulphur among the "Metallic Elements" (p. 84).

The first error which we shall notice occurs at p. 95: "Thus oil of vitriol, being composed of three charges of oxygen, united to one of sulphur and ten of water, which last is itself supposed to be composed of a single charge each of oxygen and hydrogen, the compound is expressed thus: $S + O\,O\,O + 10\ (H + O)$; or more concisely, thus, $S\,O^3 + 10\ (H\ O)$; or still more concisely, thus, $S^3 + 10\ H^1$." This passage we have quoted somewhat at length, because it proves incontestably, that Mr. Gray is ignorant of the composition of sulphuric acid; for he has once in words, and three times by symbols, misstated its nature. It may, perhaps, be requisite to observe, that by the word *charge* Mr. Gray means what other chemists term atom, proportional or prime; but oil of vitriol instead of containing ten charges of water, contains only one, as may be seen in any

modern work of the "pure chemists." It might be supposed that the word *ten*, was accidentally substituted for *one*, of water; but the symbols agreeing with the former, we must admit what we find so often repeated, to express the state of the author's knowledge on the subject, and it is astonishing that it should be so erroneous.

In p. 177, Mr. Gray states, that from "the quantity of ammonia required for saturating acids, it may in consequence of this law be inferred that ammonia contains about 46 per cent. of oxygen; and its change into azotic gas and hydrogen gas, by being passed through a red-hot tube, shows that this is united with 36 of nitricum and 18 of hydrogen; so that the composition of ammonia is 6 H + N + O." If there be any thing in chemical science which appears to be settled, it is, that ammonia contains no oxygen; and the view which Mr. Gray has given of its composition is calculated to puzzle much more than to inform. If the student after reading this passage were to look into the chemical works of Thomson, Henry, or Brande, he would find no mention either of oxygen or nitricum existing in ammonia. These speculations of Berzelius respecting the compound nature of azote, should have found no place in an elementary work.

The statements of the nature and atomic constitution of the various salts are such as will give the pupil no idea of their composition; thus in p. 204, we are told that "acetic acid, boiled on about one-quarter of its weight of litharge to three-quarters its bulk, then set by to settle and poured off clear, is the liquor plumbi acetatis of the Pharmacopœia, a dram of which added to a pint of water, and a dram measure of proof spirit, forms the well-known Goulard's lotion. These are solutions of a salt which may be crystallized in plates, and is a subtritacetate of lead, or acet. ac. + 3 ox. lead."

The fact is, that this solution is a subbinacetate of lead, but if it were what Mr. Gray represents it to be, what idea of the proportions of its constituents can the pupil acquire, without knowing the weight of the atoms of acetic acid and oxide of lead? The composition of 100 parts ought to have been stated in the usual way; added to this when treating of the nomenclature of salts in p. 81, no rules are given for describing those which contain an excess of base. On the same ground we object to the following statement: "Moist iodine added to phosphorus yields a sour colourless gas, which is rapidly absorbed by water, and must be collected in a quicksilver apparatus; a gallon of this gas weighs about 311 grains. Here the changes are either $I^3 + P$ into $I^2 + P^1$, or $I + P + H^1$ into $I\,H + P^1$; and the new acid is called the iodic or hydroiodic." The pupil would naturally suppose, that Mr. Gray considers the iodic and hydroiodic acids (properly hydriodic) as similar; but he ought to have known, that iodic acid consists of oxygen and

iodine, and the hydriodic acid of hydrogen and iodine; it is the latter only which is formed, excepting a quantity of phosphorous acid, of which no notice is taken, nor is the decomposition of he water even hinted at, although the formation of the hydriodic acid depends upon it (p. 166).

The directions for detecting the presence of arsenious acid (p. 151) are thus given in eight lines:—"If a person is suspected to be poisoned with arsenic, the antidote that is most readily obtained is a solution of soap; and the contents of the stomach may, to obtain satisfaction, be dissolved in boiling distilled water, the solution strained, and then, if any white arsenic has been taken up, on the surface being touched with a stick of lunar caustic, a sulphur-yellow precipitate will fall down immediately from the place touched." These directions are incorrect, and totally inefficient; for it is necessary to make use of solution of an alkali, either carbonate of potash as proposed by Mr. Hume, or ammonia as preferred by Dr. Marcet. Besides this omission, Mr. G. has not stated one word of the ambiguity which may arise from the presence of a phosphoric salt, nor does he give any directions for procuring the confirmatory evidence which may be obtained by the use of sulphuretted hydrogen, sulphate of copper, or from the alliaceous smell; nor is the direct evidence afforded by metallization in any way alluded to.

We cannot help noticing the contemptuous and unwarrantable language which Mr. Gray employs when speaking of Lavoisier, a philosopher to whom every one, but Mr. Gray, knows that science is deeply indebted, and whose misfortunes entitle his memory to respect. "Lavoisier reversed the analogy, and instead of continuing to identify the metallic oxides with the earths, compared the earths to the metallic oxides; and, being a Frenchman, he of course claimed this mere shifting of the terms of the analogy, as a great discovery."

With one more quotation, we shall conclude our notice of the Elements of Pharmacy. In page 88, some of the properties of azote, chlorine, and iodine, are mentioned, and then come the following observations: "All of these are esteemed by Sir H. Davy, Brande, and the chemists of that school, as simple bodies in the present state of our knowledge, but Berzelius and the rationalists consider them as oxides; the supposed bases of the two first being called by him nitricum, muriaticum, and that of the third may be distinguished by the name of iodium." Thus we have an author who has four times misstated the composition of sulphuric acid, venturing to divide chemists into the two classes of the rationalists and irrationalists, and placing among the latter the inventor of the safety lamp.—*Edit.*

Article XV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *On the Ignition of Platina, &c. by Hydrogen Gas.*

I have, says Prof. Dœbereiner, already proved that the protoxide of platinum obtained by Edmund Davy's method, has the property of causing alcohol, placed in contact with it, to attract oxygen gas, and to become converted into acetic acid and water; and that this property is likewise possessed by the oxidized sulphuret of platinum, prepared by treating a solution of that metal with sulphureted hydrogen, and exposing in a dry state the sulphuret formed by that means, to the action of atmospheric air for some weeks. In this very remarkable process, 1 atom (= 46) of alcohol combines with 4 atoms ($= 4 \times 8 = 32$) of oxygen, and forms with it 1 atom (= 51) of acetic acid, and 3 atoms ($= 3 \times 9 = 27$) of water; that is so say, equal volumes of the vapour of alcohol and oxygen gas, become equal volumes of acetic acid and aqueous vapour; for 1 atom of water is requisite to the isolated existence of acetic acid. The respective proportions in which acetic acid and water appear in this case, are exactly the same as those which they bear to each other in crystallized sugar of lead, and also in the subacetate of copper; the quantity of water in acetate of soda is exactly double that which is contained in each of the former acetates.

After having finished my experiments on this process of the formation of acetic acid, I took the opportunity of ascertaining the relations of the two above-named preparations of platinum to different elastic fluids. The results of the experiments instituted for that purpose are interesting; for I found,

1. That neither oxygen nor carbonic acid gas was absorbed by the protoxide, or by the oxidized sulphuret of platinum; but that those substances absorbed every inflammable gas.

2. That 100 grains of protoxide of platinum absorb from 15 to 20 cubic inches of hydrogen gas, during which absorption so much caloric is evolved, that the protoxide becomes ignited, and the hydrogen burns with detonation, if it had been previously mixed with oxygen or with atmospheric air.

The preparation of platinum, charged with hydrogen, has the property of greedily attracting as much oxygen gas as is requisite for the saturation of the hydrogen it contains. If atmospheric air, therefore, be suffered to enter the tube containing it, it instantly deprives it of its oxygen, and even forms ammonia with a portion of the residual nitrogen, if there be not sufficient oxygen present for its saturation. By this agency the oxide of platinum is reduced, and thereby loses its remarkable property of disposing alcohol to become acetic acid, and also that of condensing hydrogen gas; but, what is very remarkable, it retains the property of determining the latter substance to the state in which it combines with oxygen gas, and becomes water; and so much heat is evolved during this combination, that if the hydrogen gas be mixed with pure oxygen, and the volume of the

mixture be rather large, the platinum becomes red-hot. I could not but conclude, from this most remarkable phænomenon, that the finely-divided metallic platinum which is produced by the igneous decomposition of the ammonia-muriate, would perhaps exhibit this singular effect upon the detonating mixture; and, to my great satisfaction, this supposition was confirmed by the experiment. Some platinum powder, prepared from the saline precipitate just named, was wrapped up in white blotting-paper, and brought into contact with the hydrogen gas; and, as might be expected, no absorption took place, nor any other perceptible mutual action. Upon this I caused atmospheric air to have access to the platinum powder in contact with the hydrogen, and after the lapse of a few moments that remarkable reaction took place; viz. the gas diminished in volume; and in ten minutes all the oxygen of the atmospheric air admitted had condensed with the hydrogen into water. I afterwards mixed pure oxygen gas with the hydrogen in contact with the platinum; a condensation of both immediately took place, and the platinum heated to such a degree, that the paper in which it was wrapped was suddenly charred. These experiments were repeated about thirty times on the same day, July 27, 1823, on which I discovered this remarkable phænomenon, and with the same success every time.

What useful applications of this discovery may be made in oxymetry, the synthesis of water, &c., I shall hereafter state more circumstantially. I shall at present merely observe, in conclusion, that the entire phænomenon must be considered as an electric one, that the hydrogen and platinum form a voltaic combination, in which the former represents the zinc;—the first established instance of an electric alternation formed by an elastic fluid and a solid substance; the application of which will lead to further discoveries.

I obtained another interesting result in an experiment on the relation of the oxidized sulphuret of platinum to carbonic oxide. I found that this gas is always diminished to half its bulk when it comes into contact with the sulphuret, and that the remaining gas is not carbonic oxide, but carbonic acid. *The carbonic oxide gas is therefore decarbonized by the oxidized sulphuret of platinum, and thereby changed into carbonic acid.*

SUPPLEMENT.*

I send you a short supplement to the paper communicated to you some days ago, on the newly discovered properties of several preparations of platinum. That the continuation of the experiments on this interesting subject would lead to new discoveries, was to be expected. I merely mention to-day, that I have succeeded in making the observed dynamic relation of the platinum powder to the hydrogen gas, appear in a very splendid manner by experiment. If hydrogen gas be suffered to issue from a gasometer through a capillary tube bent downwards, upon the platinum contained in a small glass funnel sealed at the bottom, so that the stream may mix with the atmospheric air before it comes in contact with the platinum, which is effected when the tube is from 1 to 1½ or 2 inches distant from the platinum, the latter almost instantly becomes red- and white-hot, and

* From a letter of Professor Dœbereiner to Professor Schweigger, dated Jena, August 3, 1823.

remains so, as long as the hydrogen continues to flow upon it. If the stream of gas be strong, it becomes inflamed, particularly if it has already been mixed in the reservoir with some atmospheric air. This experiment is very surprising, and astonishes every beholder, when he is informed, that it is the result of the dynamic reaction of two species of matter, one of which is the lightest and the other the most ponderous of all known bodies. That I have already applied this new discovery to the formation of a new apparatus for procuring fire, and of a new lamp; and that I shall avail myself of it for much more important purposes, you may well suppose beforehand:—more of it in my next.—(Phil. Mag. vol. lxii. p. 289, from Schweigger's Journal.)

From the Annales de Chimie et de Physique, t. xxiv. p. 91, we extract the following additional experiments by M. Dœbereiner:—

I have found that the combustible energy of hydrogen is so much increased by contact with the powder of platina, that it will combine in a few minutes with all the oxygen of a mixture which consisted of 99 parts of azote and 1 of oxygen; an effect which cannot be produced by the strongest electrical sparks. I mix, however, for these experiments, the powder of platina with potters' clay, and I moisten this mixture to form it into small balls of the size of a pea; I suffer these balls to dry in the air, and afterwards heat them to redness in an enameller's lamp. A ball of platina of this kind, although weighing only from 2 to 4 or 6 grains, is capable of converting any volume of the detonating gas into water, provided that after each operation it is carefully dried, and it may be employed for the same purpose more than a thousand times.

The compound gases containing hydrogen, such as ammonia, olefiant gas, carbureted hydrogen, muriatic acid gas, &c. do not combine with oxygen by the intervention of the powder of platina.

When a jet of hydrogen was directed upon a mixture of powder of platina and nitrate of platina and ammonia, the mixture became red-hot with a crackling noise and the emission of sparks. The same effect occurred with the black powder of platina, which zinc separates from the solution of that metal. This powder is a mixture of oxide and reduced platina. This powder possesses the property of gradually converting alcohol, when oxygen is present, into acetic acid.

Among the other metals which I have hitherto tried, nickel, prepared by decomposing the oxalate, is the only one which has the property of converting a mixture of oxygen and hydrogen into water, and this takes place very slowly.

II. *On the Ignition of Platina by Hydrogen Gas.* By Mr. A. Garden.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Oxford-street, Nov.* 20, 1823.

The very curious phænomenon, recently observed by Dœbereiner, that a jet of hydrogen gas when brought into contact with metallic platina at common temperatures, produces a temperature equal to that of ignition, has already been noticed by other chemists, namely, by Messrs. Dulong and Thenard, in France; and by Faraday and

Herapath, in England: but these philosophers do not mention that any other than substances actually in the metallic state are capable of exhibiting a similar appearance.

After repeating several of the experiments already published, I was induced to submit a number of other bodies to the action of the hydrogen jet. Some I found to have their temperature slightly increased, and the greater number not at all: but the most remarkable increase which I have observed has been with the ore of iridium;* this substance, when previously heated to redness and suffered to cool, becomes red-hot by a stream of cold hydrogen, in the manner of spongy platina, and appears to retain the property of so doing equally well.

The circumstance of these bodies becoming heated to incandescence in our atmosphere of medium temperature, naturally suggests the idea of employing them for the instantaneous production of fire and light;† but, in order that this may be done with tolerable certainty, so as to be really useful, it becomes necessary that the effect shall take place at pretty low temperatures. To ascertain this point I made the following experiments:—

A quart bottle filled with hydrogen gas was placed in an earthenware wine-cooler, and the space between the sides of the bottle and of the cooler was filled up with ice, broken into small fragments, a small piece of spongy platina was exposed upon a slip of foil of the same metal, and laid upon the surface of the ice; in this state the whole was left in an apartment (at 52°) for about three quarters of the hour; at the end of this time the temperature of the platina foil was found to be 35°, which, with the spongy metal, was covered with a considerable film of moisture.

A jet of gas was now made to pass from the bottle through a capillary tube upon the spongy platina, the moisture immediately began to evaporate, and the metal quickly became heated to whiteness, kindling the hydrogen as it issued from the orifice of the tube.

From the result of this experiment (which was made, not so much with a view to determine the minimum temperature at which the effect could be produced, as to see whether it would take place at the usual degrees of atmospheric temperature in this climate,) it has appeared that a very ready and elegant mode of obtaining light may be obtained.

I have constructed several lamps for the purpose upon a very simple principle, and from the certainty which I have hitherto observed, I have reason to believe that they will answer most completely. When I have satisfied myself as to the most convenient form, I shall probably trouble you with a sketch of it, and also with the results of a few more experiments upon the subject.

I remain, dear Sir, your's truly,

A. Garden.

* I mean the black powder which remains after the action of nitro-muriatic acid upon crude platinum, and which also contains osmium.

† Dœbereiner says, that he has already applied his discovery to this purpose.

III. *On the Fusion of Charcoal, Graphite, Anthracite, and the Diamond.* By Professor Silliman.

(*Concluded from* p. 316.)

In a second letter immediately succeeding that already given, dated April 15, 1823, Dr. Silliman states:—

Having last year caused to be constructed an aparatus, capable of containing fifty-two gallons of gas, for the supply of your compound, or oxy-hydrogen blow-pipe, and capable of receiving a strong impulse from pressure, I have been intending, as soon as practicable, to subject the diamond and the anthracite to its intense heat. Although their being non-conductors, would be no impediment to the action of the blow-pipe flame on them, still, obvious considerations have always made me consider the success of such experiments as very doubtful. I allude, of course, to the combustibility of these bodies, from which we might expect that they would be dissipated by a flame sustained by oxygen gas.

My first trials were made by placing small diamonds in a cavity in charcoal, but the support was, in every instance, so rapidly consumed, that the diamonds were speedily displaced by the current of gas. I next made a chink in a piece of solid quick lime, and crowded the diamond into it; this proved a very good support; but the effulgence of light was so dazzling, that, although through green glasses I could steadily inspect the focus, it was impossible to distinguish the diamond in the perfect solar brightness. This mode of conducting the experiment, proved, however, perfectly manageable; and a large dish, placed beneath, secured the diamonds from being lost (an accident which I had more than once met with), when suddenly displaced by the current of gas; as, however, the support was not combustible, it remained permanent, except that it was melted in the whole region of the flame, and covered with a perfect white enamel of vitreous lime. The experiments were frequently suspended, to examine the effect on the diamonds. They were found to be rapidly consumed, wasting so fast, that it was necessary, in order to examine them, to remove them from the heat, at very short intervals. They exhibited, however, marks of *incipient fusion.* My experiments were performed upon small wrought diamonds, on which there were numerous polished facets, presenting extremely sharp and well-defined solid edges and angles. These edges and angles were always rounded and generally obliterated. The whole surface of the diamond lost its continuity, and its lustre was much impaired; it exhibited innumerable very minute indentations, and intermediate and corresponding salient points; the whole presenting the appearance of having been superficially softened, and indented by the current of gas, or perhaps of having had its surface unequally removed, by the combustion. In various places, near the edges, the diamond was consumed, with deep indendations, and occasionally where a fragment had snapped off, by decrepitation, it disclosed a conchoidal fracture and a vitreous lustre. These results were nearly uniform, in various trials; and every thing seems to indicate that were the diamond a good conductor, it would be melted by the deflagrator; and were it incombustible, a globule would be obtained by the compound blow-pipe.

In one experiment, in which I used a support of plumbago, there

were some interesting varieties in the phenomena. The plumbago being a conductor, the light did not accumulate as it did when the support was lime, but permitted me distinctly to see the diamond through the whole experiment. It was consumed with great rapidity; a delicate halo of bluish light, clearly distinguishable from the blow-pipe flame, hovered over it; the surface appeared as if softened, numerous distinct but very minute scintillations were darted from it in every direction, and I could see the minute cavities and projections which I have mentioned forming every instant. In this experiment I gave the diamond but one heat of about a minute; but on examining it with a magnifier, I was much surprised to find that only a very thin layer of the gem, not much thicker than writing paper, remained, the rest having been burnt.*

I subjected the anthracite of Wilkesbarre, Penn, to similar trials, and by heating it very gradually, its decrepitation was obviated. It was consumed with almost as much rapidity as the diamond; but exhibited, during the action of the heat, an evident appearance of being superficially softened; I could also distinctly see, in the midst of the intense glare of light, very minute globules forming upon the surface. These, when examined by a magnifier, proved to be perfectly white and limpid; and the whole surface of the anthracite exhibited, like the diamond, only with more distinctness, cavities and projections united by flowing lines, and covered with a black varnish, exactly like some of the volcanic slags and semi-vitrifications. The remark already made, respecting the diamond, appears to be equally applicable to the anthracite, i. e. that its want of conducting power is the reason why it is not melted by the deflagrator, and its combustibility is the sole obstacle to its *complete* fusion by the compound blow-pipe.

I next subjected a parallelopiped of plumbago to the compound flame. It was consumed with considerable rapidity, but presented at the same time, numerous globules of melted matter, clearly distinguishable by the naked eye; and when the piece was afterwards examined, with a good glass, it was found richly adorned with numerous perfectly white and transparent spheres, connected also by white lines of the same matter, covering the greater part of the surface, for the space of half an inch at and around the point, and presenting a beautiful contrast with the plumbago beneath, like that of a white enamel upon a black ground.

In subsequent trials, upon pieces from various localities, foreign and domestic (confined however to very pure specimens), I obtained still more decided results; the white transparent globules became very numerous, and as large as small shot; they scratched window glass—were tasteless—harsh when crushed between the teeth, and they were

* In Tilloch's Phil. Mag. for November 1821, vol. lviii. p. 386, I observe the following notice by Mr. John Murray:—" By repeatedly exposing a diamond to the action of the oxy-hydrogen blow-pipe in a nidus of *magnesia*, it became as *black* as charcoal, and split into fragments which displayed the *conchoidal* fracture.

" It will be found, that this gem affixed in magnesia soon flies off in minute fragments, exhibiting the impress of the conchoidal form.

" In lately exposing the diamond fixed on a support of pipe-clay, to the ignited gas, I succeeded in completely *indenting* it:—examined it after the experiments, it exhibited proofs of having undergone *fusion*."

not magnetic. They very much resembled melted sileago, had not be supposed to be derived from impurities in the plumbx, and might their appearance been uniform in the different varieties of that substance, whose analysis has never, I believe, presented any *combined* silex; and neither good magnifiers, nor friction of the powder between the fingers, could discover the slightest trace of any foreign substance in these specimens. Add to this, in different experiments, I obtained very numerous perfectly black globules on the same pieces which afforded the white ones. In one instance they covered an inch in length, all around; many of them were as large as common shot; and they had all the lustre and brilliancy of the most perfect black enamel. Among them were observed, here and there, globules of the lighter coloured varieties. In one instance the entire end of the parallelopiped of plumbago was occupied by a single black globule. The dark ones were uniformly attracted by the magnet, and I think were rather more sensible to it than the plumbago, which had been ignited, but not melted. We know how easily, in substances containing iron, the magnetic susceptibility is changed by slight variations of temperature. I am aware, however, that the dark globules may contain more iron than the plumbago from which they were derived, as the combustion of part of the carbon may have somewhat diminished the proportion of that substance. I find that the fusion of the plumbago by the compound blow-pipe is by no means difficult: and the instrument being in good order, good results may be anticipated with certainty. As the press is waiting while I write, it is not in my power to determine the nature of all of these various coloured globules, and particularly to ascertain whether the abundant white globules are owing to earths combined with the plumbago, or whether they are a different form of carbon. If the former be true, it proves that no existing analysis of plumbago can be correct, and would still leave the remarkable white fume, so abundantly exhaled between the poles of the deflagrator, and so rapidly transferred from the copper to the zinc pole, entirely unaccounted for. I would add, that *for the mere fusion* of plumbago, the blowpipe is much preferable to the deflagrator; but a variety of interesting phenomena in relation to both plumbago and charcoal are exhibited by the latter, and not by the former.

A postscript to this communication, dated April 18, gives the following statement:—

The anthracite of Rhode-Island is thought to be very pure. Dr. William Meade (see Bruce's Journal, p. 36), estimates its proportion of carbon at ninety-four per cent. This anthracite I have just succeeded in melting by the compound blow-pipe. It gives large brilliant black globules, not attractable by the magnet, but in other respects not to be distinguished from the dark globules of melted plumbago. The experiment was entirely successful in every trial; and the great number of the globules, and their evident flow from, and connexion with, the entire mass, permitted no doubt as to their being really the melted anthracite.

The Kilkenny coal gave only white and transparent globules; but it seems rather difficult to impute this to impurities, since this anthracite is stated to contain ninety-seven per cent. of carbon.

I have exposed a diamond this afternoon to the solar focus in a jar of pure oxygen gas, but observed no signs of fusion, nor indeed did

I expect it, but I wished to compare this old experiment with those related above.

The diamond is now the only substance which has not been perfectly melted.

I inserted a piece of plumbago into a cavity in quick lime, and succeeded in melting it down by the blow-pipe into two or three large globules, adhering into one mass, and occupying the cavity in the lime; these globules were limpid; and nothing remained of the original appearance of the plumbago except a few black points.

The subject is concluded at p. 378, of the Journal, by the additional notice subjoined, dated April 23.

If melted charcoal, plumbago, and anthracite do really approximate towards the character of diamond, we ought to expect that, in consequence of fusion, there would be a diminution of conducting power, with respect both to heat and to electricity. This I find to be the fact. As soon as the point of charcoal is fused by the deflagrator, the power of the instrument is very much impeded by it; but as soon as the melted portion is removed, the remaining charcoal conducts as well as before; and so on, for any number of repetitions of the experiment, with the same pieces of charcoal.

The globules of melted plumbago are absolute non-conductors, as strictly so as the diamond. This fact is very pleasingly exhibited, when a point of prepared charcoal, connected with the zinc pole of the deflagrator, is made to touch a globule of *melted* plumbago, however small, still adhering to a parallelopiped of plumbago, in its natural state, screwed into the vice connected with the copper pole; not the minutest spark will pass; but if the charcoal point be moved, ever so little aside, so as to touch the plumbago in its common state, or even that which has been ignited, without being fused, a vivid spark will instantly pass. This fact is the more remarkable, because it is equally true of the intensely black globules which are sensibly magnetic, and therefore contain iron, as of the light coloured and limpid ones, which are not attractable.

The globules of melted anthracite are also perfect non-conductors. This may appear the less remarkable, because the anthracite itself is scarcely a conductor; at least, this is the common opinion; and it certainly is strictly true of that of Wilkesbarre and of that of Kilkenny; for when both poles are tipped with those substances, there is only a minute spark, which is but little augmented when charcoal terminates one of the poles. But the fact is remarkably the reverse with the *Rhode-Island* anthracite; *this* conducts quite as well as plumbago, and I think even better, giving a very intense light, and bright scintillations. I have now no doubt that the deflagrator will melt it, but have not had time to complete the trial.

If it should be said that the conducting power of the Rhode-Island anthracite may be owing to iron, we are only the more embarrassed to account for the fact, that its black melted globules are insensible to the magnet, and are perfect non-conductors.

It will now probably not be deemed extravagant, if we conclude that our melted carbonaceous substances approximate very nearly to the condition of diamond.

Article XVI.

NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Capt. Parry's Journal of a Second Voyage for the Discovery of the North-west Passage. 4to.

Economy of the Eyes, being Precepts for the Improvement and Preservation of the Sight. By W. Kitchener, MD.

Mr. John Curtis has in the press No. I. of his Illustrations of English Insects. We understand the intention of the Author is to publish highly finished figures of such species of insects (with the plants upon which they are found) as constitute the British genera, with accurate representations of the parts on which the characters are founded, and descriptive letter-press to each plate, giving as far as possible the habits and economy of the subjects selected. The work will be published monthly, to commence the 1st of January, 1824.

JUST PUBLISHED.

No. XX. of Sowerby's Genera of Shells, containing Cardita, Cypricardia, Thecidium, Rostellaria, Strombus, and Pteroceras.

Chemical Recreations, a Series of amusing and instructive Experiments, &c. with a Description of a cheap and simple Apparatus. 18mo. 3*s.*

First Steps to Botany, intended as popular Illustrations of the Science, leading to its Study as a Branch of general Education. By James L. Drummond, MD. 12mo. 100 cuts, 9*s.*

Lectures on the general Structure of the Human Body, and on the Anatomy and Functions of the Skin. By T. Chevalier, FRS. &c. 8vo. 12*s.*

The Pupil's Pharmacopœia. 18mo.

Observations and Commentaries on Medicine compared as a Science with the other learned Professions. By Adam Dods, MD. 8vo. 2*s.* 6*d*

Article XVII.

NEW PATENTS.

J. Christie, of Mark-lane, London, merchant, and T. Harper, of Tamworth, Staffordshire, merchant, for their improved method of combining and using fuel in stoves, furnaces, boilers, and steam-engines.—Oct. 9.

J. R. Cottor, of Castle Magnor, near Mallow, in the county of Cork, for certain improvements on wind musical instruments.—Oct. 9.

J. Henfrey, of Little Henry-street, Waterloo-road, Surrey, engineer, and A. Applegath, of Duke-street, Stamford-street, Blackfriars, Surrey, printer, for certain machinery for casting types.—Oct. 9.

E. S. Swaine, of Bucklersbury, for a method of producing and preserving artificial mineral waters, and for machinery to effect the same. —Oct. 9.

Article XVIII.

METEOROLOGICAL TABLE.

1823.	Wind.	Barometer. Max.	Min.	Thermometer. Max.	Min.	Evap.	Rain.	Daniell's hyg. at noon.
10th Mon.								
Oct. 1	Var.	29·49	28·84	60	30	—	25	
2	N W	29·96	29·48	60	28	—		
3	W	30·08	29·96	60	40	—	08	
4	N W	30·21	30·08	59	34	—		
5	S E	30·22	29·98	65	52	—		
6	S E	30·04	29·95	62	50	—	08	
7	S W	30·12	30·04	62	36	—		
8	S W	30·04	29·68	61	46	—	21	
9	S W	29·68	29·58	56	34	—		
10	S W	29·58	29·11	55	38	—	23	
11	S W	29·30	29·11	55	36	—	10	
12	S W	29·33	29·30	54	43	·70	05	
13	E	29·57	29·33	55	32	—	04	
14	W	29·57	29·56	53	36	—		
15	S W	29·70	29·56	55	37	—		
16	W	29·73	29·70	55	30	—		
17	N E	29·73	29·67	55	38	—		
18	N	29·77	29·67	53	38	—	03	
19	E	30·08	29·77	61	50	—		
20	E	30·32	30·08	62	52	—		
21	E	30·32	30·28	60	50	—		
22	S E	30·28	30·13	55	36	—		
23	E	30·13	30·12	56	39	—		
24	E	30·37	30·12	56	37	—		
25	N	30·52	30·37	50	37	—		
26	N	30·52	30·37	49	44	—		
27	N W	30·27	30·00	55	44	—		
28	S W	30·00	29·77	58	40	—	26	
29	W	29·77	29·42	48	41	—	—	
30	N E	29·42	29·18	46	39	—	1·24	
31	N E	29·86	29·18	43	34	·70	43	
		30·52	28·84	65	28	1·40	3·00	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Tenth Month.—1. Rainy. 2. Very foggy morning: fine day. 3. White frost: day fine: evening rainy. 4. Cloudy. 5. Rainy. 6. Rainy. 7—10. Fine. 11. Cloudy. 12. Fine. 13. Rainy. 14—17. Fine. 18. Rainy. 19—20. Overcast. 21—24. Fine. 25—27. Overcast. 28. Fine: rain at night. 29. Fine. 30. Rainy. 31. Rainy: stormy.

RESULTS.

Winds: N, 3; NE, 3; E, 6; SE, 3; SW, 8; W, 4; NW, 3; Var. 1.

Barometer: Mean height

For the month	29·828 inches.
For the lunar period, ending the 26th	29·668
For 12 days, ending the 2d (moon north)	29·761
For 14 days, ending the 16th (moon south)	29·718
For 13 days, ending the 29th (moon north)	30·071

Thermometer: Mean height

For the month	47·677°
For the lunar period	48·172
For days, the sun in Libra	49·600

Evaporation	1·40 in.
Rain	3·00

Laboratory, Stratford, Eleventh Month, 21, 1823. R. HOWARD.

INDEX.

G.

H.

I.

K.

L.

M.

N.

O.

P.

V. and U.

W.

Z.

END OF VOL. VI.

C. Baldwin, Printer,
New Bridge-street, London.